EXHIBIT 4

THE U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
PUBLIC HEALTH SERVICE
AGENCY FOR TOXIC SUBSTANCES AND DISEASE REGISTRY

convenes the

EXPERT PEER REVIEW PANEL

ATSDR'S HISTORICAL RECONSTRUCTION ANALYSIS

CAMP LEJEUNE, NORTH CAROLINA

VOLUME I

The verbatim transcript of the meeting of the Peer Review Panel, held at 1825 Century Boulevard, Room 1A/B, Atlanta, Georgia, on Monday, March 28, 2005, taken by Diane Gaffoglio, Certified Merit Court Reporter.

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C O N T E N T S

Volume I
March 28, 2005

Legend of the transcript:

[sic]	Exactly as said
[phonetic]	Exact spelling unknown
--	Break in speech continuity
...	Trailing speech or omission when reading written material
[inaudible]	Mechanical or speaker failure
[microphone]	Speaker is off microphone

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P R O C E E D I N G S

8:38 a.m.

MR. MASLIA: Good morning. Welcome, everybody, to our expert panel meeting. We're going to wait a few minutes for some other people to arrive that are part of the program this morning. But in the meantime, I thought I would go through some housekeeping rules, if that's okay with everybody. And just to our panel members and everybody else that had to fly in, either yesterday or this morning, through the weather, thank you for making the effort. We appreciate it.

And -- so real briefly, for those not familiar with ATSDR campus, we're right over here. And there's a cafeteria here and down here as well is the restaurant in the Century Center hotel plus some other restaurants around. And so, on campus, there's two cafeterias and the restaurant. There will be two buses for lunch from the hotel. We've made arrangements to eat at the restaurant or the dining area at the Century Center hotel.

And I'm going to ask for those other guests, the nonpanelists, to allow the panelists to take the first bus -- it holds 12 -- so they can get to the business of eating and getting back. And then there's a second bus that will take anyone else to that, or you're free to go any place off-campus. There's a variety of foods and

other establishments.

Located on the first floor behind the guard station through the metal detector that you passed through are restrooms and candy machines and Coke machines, if the bottled water or the candy that Ann brought will not suffice.

Messages will be at a board near the registration desk, if you need someone to -- if you've got messages. And there's also a telephone out in the outer alcove for you to use. And any copying, faxing, or other needs, Ann Walker, who's staying by the door right there, and Joann -- I don't see Joann. She's out in the hallway -- Joann Flesner have been very gracious to stand by at a moment's notice and at the panel's needs to do anything you need.

And you are being recorded, audiotaped. So we ask you to speak into the microphones, primarily for the purpose so ATSDR can have a transcript and a report of your comments so we can deal with them directly after the meeting. There will be a report published of this meeting; not the transcript, but a summary report that will be available to everybody. And we're asking you to silence your cell phones. If you can, just turn them off, which would be our preference. If you have it on vibrate and you're at a microphone, everyone will hear the vibration go off. And for those in the audience, the

microphones and the court reporter can pick up your side conversation, even though you're not on mike. So I'll just remind you of that, that it will be picked up.

And with that, that's -- any other questions or housekeeping issues? If not, Dr. Sinks, are you prepared? It's my pleasure to introduce Dr. Tom Sinks, who is our director of science and acting administrator for ATSDR.

DR. SINKS: Thanks, Morris. Well, good morning to all of you. It's a pleasure to be here. As Morris indicated, I'm the acting director for both ATSDR and the National Center for Environmental Health, a title I've been -- I've had for all of three weeks. And as actings go, that may be a record. Who knows? It could be two more days; it could be two more months. But it's actually -- it's been thrilling, embarrassing, exciting. It's been -- it's been a good ride so far in three weeks.

This is a -- this is a great opportunity for us to, I think, do what ATSDR wants to be doing in these very complex sites that we deal with. And the three things, I think, we really want to accomplish here is to make sure that we challenge ourselves to do the best science that we can in what, in this particular example, is a very complex, difficult study that we're trying to conduct.

And in this case, it's the modeling of drinking water supplied to people who were living at Camp Lejeune many,

many years ago and trying to recreate exposure scenarios, which have occurred pretty far in the past; to do it in a scientifically credible way; and make it as valid as we can. And reconstructing these types of scenarios are quite difficult, and we do need help in trying to do that.

So the first thing is the best science. The second thing is trying to do this in a fairly transparent process, to be open to criticism, constructive comments, to let people know what it is that we are trying to accomplish, and to give them that idea upfront so that when we arrive at our conclusions, people have a good understanding of what we were doing and how we were trying to do it. And this panel is helping to play a role for us and when -- to challenge ourselves to the best job that we can.

The panel members here are nationally and internationally recognized experts in the areas of groundwater hydraulics, fate and transport analyses, water-distribution systems, numerical-modeling techniques. And we're delighted to have you-all here.

Again, our objectives are to secure from the panel members, who are not ATSDR employees but are people from outside of ATSDR, your critiques and your approaches and your recommendations for what we're about to do. This information will be made public.

Morris, will we put it on the Web site? Is that -- will the report be on the Web site?

MR. MASLIA: It's our intent to.

DR. SINKS: Okay. So it will also be open to the public just beyond this meeting. And I presume we'll put a response to the recommendations on there as well, how we're going to handle that.

My next challenge is to introduce Dr. Barry Johnson. Barry is sitting at the head of the table. He looks younger every time I see him. I think it's because he doesn't have to be the assistant administrator of ATSDR, and I think a great weight has probably come off of his shoulders. He's smiling. It's the first time I've seen him smiling in years. I tend to be chasing Barry around.

Barry -- I've known of Barry since 1985 when I became an EIS officer assigned to NIOSH. As soon as I arrived to NIOSH, Barry took off. He left NIOSH, and he went to ATSDR where he effectively really became the first assistant administrator of ATSDR, pulling it away from CDC, creating a separate agency and really building it to what it is today. Barry retired in 1986 -- no. That's the wrong date; 1999.

DR. JOHNSON: It depends on how you interpret retirement (laughter).

DR. SINKS: Barry left ATSDR in --

DR. JOHNSON: 1999.

DR. SINKS: -- 1999 and has joined the Rollins School of Public Health over on Clifton Road as an adjunct professor there. He's currently working on a lot of editorial boards. He's writing books. He has one in publication right now, and it's his job to give you-all a charge for this conference and to lead this throughout the next couple of days. I do plan to stop in from time to time during the course of the next two days. I won't be able to attend the entire meeting, but I wish you-all success in a fairly difficult and complex situation.

So thanks a lot and, Barry, I think it's all yours.

DR. JOHNSON: Thank you, Dr. Sinks, for those kind remarks and sage advice to the panel. We have a full agenda ahead of us over the next two days, building upon the direction that Dr. Sinks has provided to us. As you all know, I'm sort of a last-second fill-in for someone else, and I certainly look forward to trying to be as helpful as I can.

When Mr. Maslia called me about a week ago and said he needed a Chair, I listened. And I then reminded him of my retired status, my membership as a senior citizen, and so forth and so on. I said, "Morris, I'm willing to consider this, but there are many personal sacrifices I have to bring to your attention and -- for example,

foregoing my morning, afternoon, and early evening naps; my shawl; my warm cocoa; and, of course, the prune juice."

And he said, "Johnson, these sound more like excuses than sacrifices." And with that unassailable logic, I signed on. So I look forward to working with you over the next couple of days. Perhaps, we can get it done in a little bit less time.

The agency has asked me to present both a statement from the Chair as well as the charge to the panel. I'm assuming that you have the charge to the panel, but, I will nonetheless go through it shortly. With regard to the purpose and scope of this expert peer review panel, it is to assess ATSDR's efforts to model groundwater and water-distribution systems at the U.S. Marine Corps Base, Camp Lejeune, North Carolina.

This work includes data-collection activities, field investigations, and water-modeling activities that were performed through -- from March through December 2004. The panel is specifically charged with considering the appropriateness of ATSDR's approach, methods, and time requirements related to water-modeling activities. It is important to understand that the water-modeling activities are in the early stages of analysis; hence, the data and interpretations are subject to modifications based in part on information provided by members of this expert panel.

ATSDR expresses a commitment to weigh questions from the public and to respond to public comments and suggestions in a timely fashion. However, in order for this panel to complete its work, it must focus exclusively on water-modeling issues. Therefore, the panel will address questions and comments that pertain to the water-modeling effort. All other questions and statements will be referred to ATSDR staff for consideration and response.

In particular are -- the ATSDR contact for nonwater-modeling questions is Dr. Frank Bove and -- who will handle questions related in particular to the epidemiological work, and Mr. Morris Maslia and associates will handle the water modeling and other water-related questions.

Any reactions from the panel? Tread on any toes? You okay with that?

(No audible response)

DR. JOHNSON: I think the bottom-line message here is that this is a meeting for the next two days that's going to be focused on the water-modeling activities. I understand there have been other meetings that have focused on other things and so forth. Do you each have a copy of the charge to the panel?

(No audible response)

DR. JOHNSON: I will read most of that for -- just to

be sure that it's in the record and it's put before the public and would suggest that you follow along as I go through this.

The Agency for Toxic Substances and Disease Registry, ATSDR, is requesting the panel's opinion with respect to the following questions. ATSDR is seeking a majority opinion with opposing views. First, will ATSDR's approach of using "50-foot cell sizes" for groundwater modeling and all pipes, networks for water-distribution system models provide sufficient detail required by the epidemiological case control study? Should coarser, variable-spacing groundwater-model grids or skeletonized-pipe networks for water-distribution system models be considered in an effort to reduce the length or duration of modeling activities?

Two, is the ATSDR approach of simulating monthly conditions using water-distribution system models sound, or should ATSDR consider using a continuous simulation for the historical period; i.e., 1968 through 1985? If continuous simulation should be used, does this approach, A, increase or decrease the work effort with respect to modeling activities? B, increase or decrease the level of uncertainty and variability of simulated results?

Three, based on information provided by ATSDR to the panel, are there modifications or changes that ATSDR

should consider making in its approach to modeling, A, groundwater resources at Camp Lejeune; B, present day; i.e., 2004, and historical reconstruction of water-distribution systems serving Camp Lejeune? If, in the panel's majority opinion, ATSDR should consider changes in its approach, what specific changes does the panel suggest?

And fourth, compared with other publicly documented historical-reconstruction analyses, is the three-year project schedule for completing all historical-reconstruction modeling activities appropriate and realistic for the amount of work and level of detail required by the epi study? If, in the panel's majority opinion, ATSDR should modify the project schedule, what specific actions and activities does the panel suggest ATSDR take to modify the project schedule?

That is the charge to the panel as developed by ATSDR. Any questions or reactions at this time to either the statement or the charge to the panel? It is the Chair's intent on Day 2 to go through each of these four charges, beginning at the "working lunch" on Tuesday. And at minimum, I anticipate providing your reactions, your advice to the first two charges at the working lunch.

If we work in, perhaps, an exceptionally, efficiently way, then we might try to go through Charges 3 and 4. But

at least we'll do the first two charges tomorrow at lunch. Charges 3 and 4, if they remain unaddressed, will be subject to our discussion at the 2:30 period.

The take-home message to the expert panel is that we will provide answers to our -- the best of our ability to each of these four charges. Is that okay with the panel?

(No audible response)

DR. JOHNSON: At this time, I'd like to ask each of the panel members -- and as Dr. Sinks said, it's truly an internationally distinguished panel, and we welcome you to Atlanta. Sorry the weather wasn't a bit better, but it's that time of the year, folks, in Atlanta; pop-up storms.

I'd like to ask each of you to introduce yourself, your affiliation, experiences related to this panel's work. And I think I'll ask each of you, as you go through your introductions, to give an initial but pithy, succinct reaction to what you have read, the information that was provided to you. I'm not asking you to pass judgment at this time. That's going to be the product of our deliberations, your deliberations in particular, but just an initial reaction to what you have received. Okay.

Let's start to my right, if we could, with Dr. Walski.

DR. WALSKI: Okay. My name is Tom Walski. I'm with the Haestad Methods Group within Bentley Systems. I've

been doing water-distribution analysis work since the seventies and have worked on systems ranging from outhouses at rec areas to the New York City water-supply system. I've done some reconstruction of water quality, in one case with Ben Harding, who's showing up later on. So I have some experience in doing this kind of reconstructive work as well. And my initial pithy reaction is: Gee, I wish I had the budget that these guys had when I was doing my work.

DR. JOHNSON: Thank you. Dr. Singh.

DR. SINGH: Yes. My name is Vijay Singh. I am a faculty member at Louisiana State University. I have been involved for many, many years in hydrologic modeling, both in surface water as well as groundwater modeling. I have also been involved in this kind of analysis as well as stochastic modeling, which has involved some reconstruction work, more specifically in the area of groundwater, particularly the area of surface water as reconstruction codes.

My reaction, based on reading the reams of papers and reports that we were supplied, is a very positive one. I was much impressed with the level of effort and the scientific rigor with which the work has been done.

DR. JOHNSON: Thank you. Please.

DR. POMMERENK: My name is Peter Pommerenk. I'm with

AH Environmental Consultants. We specialize in water resources, water treatment, water distribution. In such, we are involved in water master planning and treatment studies and treatability studies. We also do some water-distribution system modeling, although we don't use Haestad methods at this time.

My particular expertise for this panel is that AH Environmental Consultants has been consulting with Camp Lejeune for several years in the water resources and treatment-distribution system arena. And we have also as such supported the Marine Corps in their efforts to collect data for this ATSDR study.

My initial reaction, when I got first involved in this project -- as I said, this is a huge effort. And what has been collected today is really impressive. Thank you.

DR. JOHNSON: Thank you. Let's just continue.

DR. CLARK: My name's Robert Clark. I spent 41 years with the federal government in the U.S. Public Health Service in the U.S. EPA as a public health service officer for 30 years. And during that time, I was director of the drinking-water research division -- water-resources research division for EPA for about 14 years and then for three years as a senior scientist in the agency and then retired in -- about three to four years ago. And since

that time, I've been consulting and am an adjunct professor at the university, which is keeping me busy as well.

Very impressive. I had a chance to work with Morris early on when he was working on the Toms River project. They've come a long ways; very impressive technical effort. I think the questions are even more challenging in terms of how can you extend this now to exposure epidemiology.

DR. DOUGHERTY: My name is Dave Dougherty. I'm from Subterranean Research in Massachusetts. I spent 15 years as a faculty member in civil and environmental engineering in California and Vermont. My background started in groundwater and moved to modeling and moved to optimization and more -- slightly more on the IT side now.

I think the things that I bring to this particular table are the integration of groundwater modeling and optimization kind of activities, experience with a lot of models in the past, and the most interesting connection is when Roger Page and I, in 1985, I think, built the first 3-D model for Toms River; so just trying to connect the loop.

My reaction is there's been a lot of -- there's been a lot of good work here. It is in many ways, in many ways, very far advanced in particular narrow areas for the

project. As a whole, I think we have a lot of opportunities to make contributions to the directions that need adjustment, and I'm looking forward to it.

DR. JOHNSON: Thank you.

DR. UBER: My name is Jim Uber. I'm an associate professor at the University of Cincinnati in the department of civil and environmental engineering. I'm an environmental engineer. My research area is water-distribution systems analysis. I've been working in that area for about 15 years and have, kind of like David, focused to some degree on optimization studies and calibration techniques for models, particularly on water-quality models for water-distribution systems and as well as doing some fieldwork and tracer tests.

And my initial reaction is that I thought that the data that was provided was very comprehensive and in particular on the water-distribution systems' side. The -- for example, the fieldwork is certainly very much state of the art in that area, and I think a central question for me is exactly how that fieldwork and those data link back to the needs of the epidemiological study and how they connect up in a logical way with the historic data that is or is not available for what happened some years ago.

DR. JOHNSON: Thank you.

DR. KONIKOW: My name is Lenny Konikow. I'm a research hydrologist with the U.S. Geological Survey. I've worked for them for over 30 years; to a large extent, working on the development and application of solute-transport models, contaminant transport models for groundwater systems. One of the first applications I was involved in was reconstructing the history of groundwater contamination at the Rocky Mountain Arsenal in Colorado, which was kind of the forerunner of the whole installation and restoration program in the Department of Defense.

One of my concerns, reading through all the documentations and thinking about this, is the lack of historical data from the fifties, sixties, on into the seventies. And I see that as presenting a very difficult hurdle to overcome in trying to develop the quantitative models. There's going to be invariably a lot of uncertainty associated with the results of the very quantitative models.

And as Jim said, I'm also a little concerned that I don't have a firm feeling yet -- and I hope I get it today -- for what -- how the models will be put to use. What is needed by the epidemiological studies to come out of the models? And for us to evaluate the models and the approach to modeling, I think we need a clearer -- or at least I need a clearer understanding of how the models are

going to be used in terms of the epidemiological studies.

DR. JOHNSON: Thank you. We have two other panelists who will be arriving a little bit later: Mr. Harding and Dr. LaBolle. Did I pronounce that correctly? We look forward to their joining us. Any questions across the table to each other?

(No audible response)

DR. JOHNSON: My hope is that this is truly an interactive panel, and I encourage dialogue, questions back and forth across the table amongst the panelists. And to the extent that I can help clarify, I will try to do that. But this is your panel, and this is your opportunity, as we've already heard, to have some concerns and some really important questions placed on the table already. So keep that up.

I think, at this time, there's going to be an introduction of the epi team and the water-modeling teams, Dr. Bove, and Mr. Maslia.

DR. RUCKART: Good morning. I'm not Dr. Bove, by the way. I'm going to be discussing a summary of ATSDR activities at Camp Lejeune and hopefully answering your question of how the water-modeling component will fit in with epi study.

DR. JOHNSON: Would you introduce yourself, please.

DR. RUCKART: Yep; next slide.

DR. JOHNSON: We'd love to know who you are.

DR. RUCKART: My name's right there. I'm Perri Ruckart. I'm the principal investigator of the epi study, and my other team members include Dr. Frank Bove, Miss Shannon Rossiter, and Dr. Morris Maslia, who I believe everyone knows.

Next slide, please.

The base began operations at Camp Lejeune in the 1940s. Currently, there's a population of about 150,000 living or working on the base, including active military personnel, their dependents, retired population, and civilian employees. Almost two-thirds of the active military personnel and their dependents are under age 25.

Next slide.

Because this is a military base, there has been considerable in-and-out migration. It is estimated that about one-third of the mothers receiving prenatal care at the base hospital during the 1970s and '80s were transferred off base before delivery, and the average duration in base-family housing is two years. There are 15 different base-housing areas. And there are three water-distribution systems serving the base-family housing area: Hadnot Point, Tarawa Terrace, and Holcomb Boulevard. And the dates they were constructed are shown here on this slide.

Underground storage tanks were installed during the 1940s and '50s, which contaminated the Hadnot Point wells, primarily, with TCE. And ABC One-Hour Cleaners began operations on the base in 1954, and the cleaners were near the supply wells for Tarawa Terrace, and that water system was primarily contaminated with PCE.

ATSDR published a public health assessment for Camp Lejeune in 1997. Because of the limited information in the scientific literature on how chlorinated solvents in drinking water might affect a fetus or a child, the public health assessment recommended that we conduct an epidemiologic study to evaluate whether maternal exposure was associated with the higher risk of having an adverse birth outcome or whether maternal or infant exposure was associated with a childhood cancer.

As a first step in following up the public health assessment recommendation, ATSDR published a study in 1998 which evaluated potential maternal exposure to drinking-water contaminants on base and preterm birth, small for gestational age, and mean birth-weight deficit. Only available databases were used, such as electronic birth certificates, which were available beginning in 1968, and base family-housing records.

There was insufficient data available for the 1998 study to evaluate fetal deaths. The study did find an

elevated risk for SGA, small for gestational age, only among male infants exposed to Hadnot Point water, which was primarily contaminated with TCE. And the study also found an elevated risk for SGA among infants born to mothers who were greater than 35 years of age and mothers with two or more prior fetal losses who were exposed to Tarawa Terrace water, which is primarily contaminated with PCE.

Because the 1998 study could not evaluate birth defects or childhood cancers, the current study will look at these outcomes, using a case control approach. It is a multistep process, and the first step involved a review of the scientific literature to identify specific birth defects and childhood cancers that were associated with drinking water contaminated with VOCs.

Next slide, please.

And this slide shows the outcome selected for further study based mainly on evidence from the epi studies of VOC-contaminated drinking water.

The second step in this process was to conduct a telephone survey to identify the potential cases of the selected birth defects and childhood cancers occurring to mothers who were pregnant at any time during their pregnancy and living at Camp Lejeune during 1968 to 1985. And the survey needed to address the questions shown here.

Can you go back for a second. Okay.

And as part of the telephone survey, ATSDR surveyed parents of 12,598 children. This is an overall participation rate of approximately 74 to 80 percent. And the survey identified sufficient numbers of neural tube defects, oral clefts, and childhood cancers. 106 cases were reported, including 35 neural tube defects, 42 oral cleft defects, and 29 childhood cancers. And the childhood cancers include leukemia and non-Hodgkin's lymphoma.

Next slide, please.

The third step is to verify the diagnoses of the reported cases. To date, 24 reported cases have been confirmed as not having the condition of interest or being ineligible or refused. That leaves us with 82 children with pending or confirmed conditions. And by pending, I mean we are still looking for evidence to verify they have their condition. That includes, for the neural tube defects, 15 confirmed as having that condition. Thirteen are still pending. For the oral clefts, 20 confirmed as having that condition and 16 still pending. And for the childhood cancers, 14 confirmed as having that condition and four still pending.

The study will include 818 controls, who were sampled from the original survey population. This is a ratio of

about ten controls to cases. Interviews will begin in the spring and continue through the summer of this year. And they will be administered to parents of the cases and controls to obtain information on maternal water-consumption habits, residential history, and parental risk factors. We anticipate a 90 percent participation rate based on previous contact with this population and the interest that they've shown in our work.

An important part of the current epi study is the water-modeling component. There's a lack of historical contaminant-specific data at Camp Lejeune. To provide a quantitative estimate of exposure, a historical-reconstruction approach is needed, consisting of modeling the groundwater flow and present-day distribution systems at Camp Lejeune and extrapolating backwards in time. The water-modeling component needs to address the following questions shown on this slide.

Next slide. Oh, go back. Can you go back, please.

DR. KONIKOW: Do you define "exposure" as just being the presence or absence of a contaminant, or are you interested in knowing the concentration of the contaminant?

DR. RUCKART: We would like to know the concentration, and our hope would be to group them into some kind of high, medium, low exposure. But it's going

to be dependent on what is available. That's our ultimate goal.

And the goals of the water-modeling component are to determine when the contamination arrived at the wells and the spatial and temporal distribution of the contaminants by housing location. And I'd like to conclude with the study time line.

Are there any questions? We'll be here throughout the panel if things should come up.

DR. JOHNSON: Could you go back, please, to the couple of slides previous; one more; stop. Thank you. No; the one that says "Current ATSDR Epi Study; that one; try again; stop. Thank you.

My question, I guess, is to Mr. Maslia. Are these questions to be addressed in the water-modeling component part of what has been put before this panel? Or are these questions that are, maybe, new?

MR. MASLIA: Part of the -- some of the questions are to be addressed by this panel. We've -- you want me to speak into the microphone, I guess. Let me just come over here and sit down.

Some of the questions have been put forth in the discussion, for example, at Tarawa Terrace where the source is located, the strength of the contaminant source. Others, for example, like at the Hadnot Point, we

obviously have not addressed that issue at this point in time. And that's an issue for us to discuss and to address, both with information that we may present or elucidate to the panel now in some of the complexities at Hadnot Point, as opposed to Tarawa Terrace.

Which chemical compounds were supplied? Again, at Tarawa Terrace, it is our intention -- and the data that we have presented has at this point indicated that PCE, PERC, is the primary contaminant, and that's what the modeling to date has been done on. We have not looked at modeling-degradation products, say, TCE to DCE and TCE.

Hadnot Point, again, presents a much more complex issue because, as Perri has alluded to, it's primarily TCE, but there was underground-storage tanks as well. And we just have not -- I'll get into -- actually, when I give an overview of the water-modeling activities as to our rationale for going in one direction right now. But we have not addressed that issue.

How was the contaminated water distributed is a main focus of our investigation. And we start out -- our approach is to try to understand what's going on today simply because of the lack of historical data, and I will get into a little bit later on our approach for deconstructing the system, if that's the way, actually, we proceed. That is, indeed, a required step that we go.

Lenny, did you have a question? Yes.

DR. KONIKOW: In terms of the water distribution and the goals of that modeling, are you aiming to actually get exposure down to the household level?

MR. MASLIA: We're aiming to get it down to the street level. Now, at Camp Lejeune, it so happens -- and we'll get into this -- the distribution is built such that it's a looped system so that each house is serviced by a pipe, as opposed to, say, an area like Dekalb County or even Toms River, where maybe there was a 4-inch main running down the street and we did not model any of the attached or smaller diameter pipes.

But the way the distribution system is constructed at Toms -- I mean, at Camp Lejeune, you really have a 2-inch pipe going from the street to the house. So in essence, by default, you've got houses attached or implied in your distribution-system modeling.

However, I think it's important also to tell the panel as well as the public is -- as with other contamination sites that we have looked at, we are actually blinded to the cases and controls at the site. ATSDR people modeling the groundwater and distribution system, we haven't been provided nor are we asking for any specific information as to who resides, who's included in the cases and controls so that it is our approach that any

models that we develop or any analyses -- let's make it more general -- should be robust enough that if you say you want Location XYZ, you should have as much confidence in the results that we give you for Location XYZ as Location ABC. And that is our approach, but we are blinded. So hopefully, that's addressed your question.

DR. BOVE: I just want to say one more thing that one of the questions earlier was: How are we going to categorize exposure? And as it was done in Toms River and Woburn, where they just focused on the percent of the water coming from a contaminated well during a month and then averaging over that for the exposure window, we'll be doing something like that. They had three categories in the Toms River study. Woburn was ever-never, and then they did have three categories, again, of exposure, the high one being the upper tenth percentile, if I remember right.

But the numbers get small when you start doing that. And I have some tables, and we can discuss the impacts of exposure misclassification bias and some of that during the panel discussion at some point during the day, if you want.

DR. JOHNSON: Yes.

DR. WALSKI: I think just to put things in perspective, you said there were about 80-some cases of

illnesses that are -- were determined in the study group. About what would the number of illnesses be out of the -- like, an average population? Would it be, like, many times above what we would expect? Or is it only marginally, or what's the perspective?

DR. BOVE: Well, part of the problem here is the way we had to ascertain cases. Ideally, you would like to have a cancer registry, or you would like to do your case ascertainments through hospital records. We had to do it through a survey. So this is not the most optimal way, but it was the only way to do ascertainment of cases. That being said -- and all the comparison data is based on medical records data or cancer registries, like the Sierra Cancer Registry, or birth defect registries, like the one in Atlanta.

It's hard to really compare the two. But if you want, these are -- what we've -- both the reported positive ones that we verified and the ones we're still working on, if you combine those two, we have slight elevations here in the -- I would say the realm of two times what we might expect for some of the end points.

But, again, there are problems with that. Not everybody was exposed at Camp Lejeune either. And the way we ascertained them was different than the databases we would compare them to.

DR. JOHNSON: Other questions? Dr. Singh.

DR. SINGH: So here the assumption was that the increase was attributed to the water contamination?

DR. BOVE: No. We didn't want to do that. We wanted to use the survey to ascertain cases and do the study with the modeling that Morris -- and you're going to be commenting on. We did not want to say straight off whether the -- it was an excess, number one, because we wanted to verify the cases. At the time of the survey, it's only self-reporting -- or parent-reported cases. And so we wanted to verify those cases.

And secondly, because of all the problems with the water information, new information we've been getting over the -- well, not so new actually, over the last few years that things we thought we knew about the water system, information we got about the water system was not quite correct and that, in fact, the study that Perri mentioned that we completed in '98 probably needs to be revisited.

Most definitely, it needs to be revisited because assumptions made in that based on that information at the time, but we find it was incorrect. So we didn't want to do anything until the modeling was done, and we -- and base whatever we do on better information.

DR. CLARK: Are we going to have a chance to look at other compounding effects?

DR. BOVE: We -- well, as Perri pointed out, we're doing an interview of the cases and controls. That's one of the nice things about doing a case-control sample. You have a small enough group so you can do extensive interviewing and go over all the other risk factors that are either suspected or known for these outcomes.

DR. JOHNSON: Do the members know the essentials of a case-control epi study? Are you-all real comfortable with that?

DR. BOVE: Well, we can -- we -- again, that's something we can go into in-depth at any point during the day.

DR. JOHNSON: Could you give us about two minutes now?

DR. BOVE: Okay; two minutes? Okay. Well, I mean, you have -- we're not sure how many pregnancies occurred at the base between 1968 and '85 because many were transferred. We had to guesstimate that about a third of the people who were pregnant there migrated off-site -- transferred basically off-site before they delivered. So we knew how many births on base. That was about 12,400 and some. And we assumed another 3,000 or so were transferred off base and delivered elsewhere, so roughly around 16,000.

Now, you have 16,000. You can't interview them all;

right? That would be an incredible undertaking. That's one approach. Another approach is to take a random sample. But when we have rare diseases, that's not a good approach because you take a random sample and may not get any of the cases in that random sample of 16,000. So the approach you take within a disease that's rare, like this situation, is what we call case-control sample.

DR. JOHNSON: You're speaking of birth defects; correct?

DR. BOVE: We're talking about birth defects. We're talking about, in particular, neural tube defects, which is spina bifida and anencephaly. We're talking about oral clefts, which is cleft lip and cleft pallet. And we're talking about childhood leukemia and childhood non-Hodgkin's lymphoma. And those are all rare events, those diseases that we're focusing on.

And so the approach has been to gather all the cases from that population at Camp Lejeune, keeping in mind that the population at Camp Lejeune of births, both born on site and born off site, some were exposed; some were not exposed; right. That's the question we're going to be asking you is hopefully is will the modeling be able to tell us with some assurance who's exposed at least and who wasn't exposed. If we can get that, that's one step.

And then, of course, we'd like to have -- be able to

define it better than that. But that's the first consideration. So we have a population here, some of whom are exposed, some of whom are not exposed during their pregnancy. And we take -- we get all the cases from that population, and then we take a random sample of that population to give us a control series. And that's the case-control series.

Now, in some methodologies, you sample your control series irrespective of whether they were -- what their disease status was. That's one approach. A lot -- most often, though, you sample the nondisease, those people in the population that did not have the case -- the diseases you're focusing on. So that's basically what we're talking about: a case-control sample, the most effective way of doing these kinds of studies. It was also the approach taken in Woburn, the approach taken at Toms River.

DR. SINGH: So why do you have some people not exposed if they were living on Camp Lejeune?

DR. BOVE: Well, we're -- see, that's the question. We -- in the previous study, we thought that about half of the births were unexposed because they were getting water from the Holcomb Boulevard system. And at that time, we assumed that the Holcomb Boulevard system was clean. Okay? So that study, half -- about half the births were

unexposed.

Now we're not sure about anything, or at least I'm not. I'm waiting to hear from the discussion. There may be interconnections between Holcomb and Tarawa Terrace. The -- before '73, the people who -- the residences that got Holcomb Boulevard water got Hadnot Point before that. And so we thought that they -- for some reason, we didn't know what their exposure was. We assumed they were unexposed. That was a bad assumption probably.

So we don't know the percent unexposed. I mean, that's what the modeling effort's going to have to tell us. That's why we have to revisit those previous -- that previous study.

DR. RUCKART: There's another piece about those also when during the pregnancy that the mother was exposed. And we're hoping to have that information as well if they were exposed in the first trimester or later. It depends on when they were actually residing at Camp Lejeune.

DR. JOHNSON: David, you had a question.

DR. DOUGHERTY: It actually follows on that one, and it is: You addressed the issue of the spatial resolution desired. What temporal resolution of exposure is desired from these studies?

DR. BOVE: Well, for neural tube defects and oral clefts, the window of exposures is the first trimester.

And actually, for neural tube defects, it's Day 20 to 24, roughly. So we're not asking for day. But we are asking for a trimester with the idea that, you know, that the exposure windows for neural tube defects and oral clefts is quite small. Okay.

Now, childhood leukemia and childhood non-Hodgkin's lymphoma, we are not sure. We -- from the studies I've seen, the initial cause for the disease appears to be prenatal. So again, we're interested in most often -- mostly in prenatal exposures for this study as a whole for all the outcomes.

DR. JOHNSON: Other questions? Yes, please.

DR. UBER: Just to -- I think I know the answer to this, but just to clarify. The study is not concerned with any fetuses that would not have made it to a live birth that might have had a cause from contamination?

DR. RUCKART: Right; because it's difficult to ascertain that. If we could, that would be ideal. But it's just not really possible here.

DR. JOHNSON: Yes.

MR. MASLIA: Just to help everybody get oriented, I think during a subsequent presentation, I've got some maps and some slides, so we're all calling the same parts of the base the same names and things like that. And we'll define that for everybody, so...

DR. JOHNSON: Thank you. Any more questions to Dr. Ruckart or Dr. Bove? I have one last question to PI. This isn't a question but a comment. The question will follow. It looks like these five questions in the main are -- have been in some way put before the panel. Do you feel that that's true? I mean, are you okay?

MR. MASLIA: Absolutely.

DR. JOHNSON: Okay. I would --

DR. RUCKART: We work together.

MR. MASLIA: We even talk with each other.

DR. JOHNSON: Lord, the agency has indeed changed since I left (laughter). I'm so glad I'm sitting down. I would invite the epi team, starting with this principal investigator, to place before this panel at any time questions that you feel have not been addressed or have not been addressed to your satisfaction because this work in terms of the water modeling absolutely has to be vital in support of your work. And now is an excellent time to get things, you know, you always wanted to ask. Put it in front of this group, and you will have profound answers.

Now my question: You mentioned work that's upcoming in the spring of 2005. Has that work begun?

DR. RUCKART: We are actually traveling up to Maryland this weekend to be part of the training for the interviewers, and interviews are scheduled to begin Monday

night or Tuesday morning by the latest. That will be next Monday and Tuesday.

DR. JOHNSON: Do you foresee anything that this panel will do over the next two days as having impact for the spring work?

DR. RUCKART: I don't believe so.

DR. JOHNSON: Okay. Well, thank you very much for your presentation. Mr. Maslia, a summary of water-modeling activities.

MR. MASLIA: Let me get the summary of water-modeling activities. Actually -- no. Let's go to project staff first; yes. Thank you. I've got it. I've got it.

DR. JOHNSON: And there are handouts here for the panel.

MR. MASLIA: The panel, yes. Some of the handouts are copies of this slide, and if any of the slides that we show that you would like copies of, please let me know or let Ann Walker know, and we'll try to provide those for you.

DR. JOHNSON: Are these available to the public outside?

MR. MASLIA: Some of them are. The ones that contain actual model simulation and data are not because they have not been cleared by the agency and subject, obviously, to panel deliberations. And so those are not available to

the public. But we do have posters and maps, showing some information that everyone's free to look at and peruse, and we'll be pointing to.

Let me officially, I suppose, introduce myself. My name's Morris Maslia. I'm a project officer of the Exposure Dose Reconstruction Program at ATSDR. And I was approached by Dr. Bove and his predecessor to take part in the Camp Lejeune epidemiologic study and looking at some of the techniques that we used for the Dover Township analyses and seeing if those, in fact, could be used or something similar to that could be used.

I've introduced myself. Also from ATSDR is Jason Sautner over here. Jason did the bulk of the modeling work at Dover Township and had his intentions on doing the modeling here. But as things progressed, Jason has really helped us developing some of the field approaches and field protocols for the tracer tests on the water-distribution system modeling and setting those up, setting up the field type of analyses and data gathering. And so he's been more involved in that respect up until this point.

We also have -- we used the Oak Ridge Institute for Science and Education to get postgraduate research fellows to assist us. Claudia Valenzuela has unfortunately been relegated to helping us with logistics on the slide screen

back there. I don't mean to point the laser at you, Claudia. It's like *Star Wars*.

But Claudia has really done the lion's share of the water-distribution system analyses that were presented in the notebooks and also has done a tremendous job in investigation in trying to figure out this issue of classification of different types of consumption and demand. We'll get into that. Obviously, being a military reservation, we may not have a simple case of residential, urban, industrial-type classifications.

Also just joining us this past October is Joe Green, and Joe's background is in medical geography. And all of the nice posters and the spatial analysis work, Joe has helped us out. He goes back and forth between the distribution-modeling results and the groundwater-modeling results, helping us put together and pull different aspects of the data.

And as far as groundwater modeling and fate and transport modeling, we have Robert Faye, who is sitting over there. And Bob spent -- and I had my notes. It's probably on another slide here but -- I believe, 27-1/2 years in U.S. Geological Survey; 12-1/2 or so, he was the regional groundwater specialist for the southeast region at USGS. And he has been doing the groundwater -- not only groundwater modeling, but the geohydrologic

framework, culling through the data files for the groundwater aspect of the analyses.

And then finally, we also have Dr. Mustafa Aral, who is sitting right at this table. And we have a cooperative agreement with the multimedia environmental simulations lab at Georgia Tech. They assisted us with our Dover Township work and are involved -- I expect to be even more involved when we start tackling this issues of uncertainty modeling, operational cycles, and things of that nature.

And finally, not present -- and I'm not sure why Dr. Grayman decided that he'd rather be on the beach at St. Maarten than here -- but Walter Grayman, whose background is in water-distribution system modeling, has been an advisor to us, helping plan the tracer tests on the water-distribution side as well as water-distribution system modeling. And as I said, he's an advisor to ATSDR.

So that is the project team. I would like to just -- and we can revisit this, but I was -- in going through some of the premeeting comments, which we really do appreciate. It helped us focus more on the direction we needed to go and some of the answers we're going to try to at least provide you in a general sense at this meeting and something to work on, obviously, after the meeting.

But a couple of questions came up with respect to the charge on the work effort. Obviously, everyone's admitted

thus far this is not a small undertaking. And so I put together a couple of slides just very quickly, and you have -- there's a -- should be a packet. If not, we can provide you these in your handout.

But this slide sort of shows -- the red bar is the total work effort, the percent of effort. You see, for example, groundwater, we're estimating thus far has taken about 35 percent of the total effort. Water-distribution system modeling is about 40, primarily because of the field and us having to go out in the field and that nature. Data discovery -- this is anything from going through the Marine Corps base facility that they call "the vault" to look through data to other -- finding other sources of information. And then communication, whether that's preparing reports for this meeting, preparing presentations, or ultimately preparing final reports or protocols as to what we did.

And just within each subject I subdivided. For example, in groundwater modeling, you've got a data discovery component and you've got a data-analysis component, which would be both geohydrologic and modeling and so forth.

You can see that in the water-distribution side, we've got an extremely driving up until this part is the, I believe, that's the data discovery. No. That's the

spatial analysis. I'm sorry; spatial analysis. And that is the cause of the complexity, both present day as well as historically, of exactly having documentation of where the pipes were, which treatment plants were operating.

A lot of this information originally was on paper copies, and we had to geocode it and all that sort of stuff. Even conducting field tests, locating hydrants, many, if not most, of the hydrants on base are not numbered. And we had to physically send people out there to actually locate and two different people locate two different hydrants and things of that nature. So that's what's driving that.

The final slide is more of a budgeting in terms of staff. If you add up all the red bars, it adds up to about four and a half equivalents, full-time equivalents. And so within that, again, you can see the present day. This refers to the present-day water-distribution system modeling. It is really driving the time-consuming and manpower-intensive aspect of the project. So that's just a very quick overview of our staffing from the water-modeling side.

And I believe that's all the project staff comments I have, unless someone has any specific questions on those. If not, I think next on is a summary of water-modeling activities. Claudia, if you will -- and I think that's

number four; number four -- no. It's number five. Yeah, yeah; right there. That's it. Okay.

I'm going to just give a very brief overview of modeling activities, so hopefully you get -- if the written documentation you were providing was confusing enough and voluminous enough to sort of simplify it. And you can go on -- I've got it right here. Okay.

Obviously, we're in coastal North Carolina, and we've got some maps here, some aerial photographs. But as Frank mentioned, there are actually seven water-distribution systems. And historically, there have been eight different water-distribution systems at Camp Lejeune. And we are actually concentrating the discussion today in our charge are the ones down in this area right over here.

So the ones, for example, at the air base, which is over here, and Onslow Beach, while they have and we may have information on them, they are not part of the analysis that we are undertaking. Basically, Perri reported this information; population of active duty, 100,000; and seven water systems supply groundwater at Camp Lejeune.

Here are the names of the different systems, and as I said, we're dealing with the Tarawa Terfrace, Holcomb Boulevard, and Hadnot Point systems. And in the next slide, what I would like to do -- and we have the posters

up, that one over there, and I think if you want to move the second poster. Okay.

We have sort of a nomenclature issue. As anybody who's done any groundwater investigation or other investigations, as you get later and later time away from either when the wells were installed or the systems operated, names change.

So this is the nomenclature that we are using for the present discussion and for the present-day system. At present, there are two operating water-treatment plants. Water-treatment plants service areas that we are analyzing. And these are the Hadnot Point, which is down to the south here. And we're referring to that as the Hadnot Point water-treatment plant service area. And then there's the Holcomb Boulevard water-treatment plant service area, which is this area.

Basically, there are two sets of shut-off valves right along the Wallace Creek here that at present day separates the two systems completely. They're shut off. In terms of actual water-distribution systems, there are three water-distribution systems within the two water-treatment plant service areas. Hadnot -- could you back up? Okay.

Hadnot Point happens to service the Hadnot Point water-distribution system area. So it's coincident. The

treatment plant services the water-distribution system. However, in this northern area, the Holcomb Boulevard actually services two different distribution systems. One is to the northwest here, the Tarawa Terrace water-distribution system, which presently is combined with service to Camp Johnson.

Historically, there was another treatment plant here, which I'll get to in a minute, and then also the distribution system at Holcomb Boulevard area. There is one pipeline here that, once the water is treated at the treatment plant, sends water to an underground reservoir at Tarawa Terrace and based on demand and tank levels would then distribute water just to the Tarawa Terrace area.

So are there any questions with respect to nomenclature that we're going to use for the balance of the panel meeting at this point?

(No audible response)

MR. MASLIA: I'll get to a very brief chronology. We've got some larger boards here. And as Frank said, this chronology has been sort of at times chasing a moving target. And so it remains sort of changing in flux even as we speak. As we get new information or as we get conflicting information, we start changing.

But very briefly, the Hadnot -- this is actually as

-- I put this together last week, so it's the most current that we have. '43, Hadnot Point was the first distribution system and first treatment plant on base. And then in '51 to '52, the Tarawa Terrace treatment plant was constructed. That's about the time that they also built the housing complex at Tarawa Terrace. And then at '50 -- in '57 was the Montford Point. And the Montford Point actually serviced the Camp Johnson, which is the northwestern-most part of the distribution system.

Then we have a big question, which we have not resolved to date yet. We cannot get a month or year as to when Holcomb Boulevard began operating. They've got a picture on the wall that says '73. You know, one of those architectural pictures that -- and we do have an accounts book that we just received a couple of weeks ago that lists when the information is filed into their system. That sort of lists '73 as well. However, documentation that we have just -- that we've just recently received says '71, and that can be a very critical issue.

So all I can say is I'm at the panel's mercy. That is a major issue, and, in fact, I think -- and I hope the panel doesn't mind me mentioning names, if you've made some comments. But Tom made a comment about putting some effort into data discovery. I'll call it that. And that still is ongoing and needs to be refined. We're planning

to do that some more, but we're going to have to obviously get detailed into the files to figure that out. So I'll just put that up there. We're not sure when in that time frame. And obviously, if the epidemiologic study is looking at months, that becomes an issue.

Tarawa Terrace -- when the water-treatment plant was closed, again, we think March. We think 1987. It started back in '85. We just recently obtained some information, a report, that I'm asking for some more background on -- that I've asked the Marine Corps for some background on that was written in '91 that makes a statement in there that, "Two years prior," which would be at -- in '89, "that Tarawa Terrace" -- and I'm quoting --- "supplied water to Holcomb Boulevard." That, again, so -- and that's in a consulting report. There may be other information as well, but that's some of the issues we're still dealing with.

And finally, in '87, again, we have some documentation that says all the remaining wells were closed. So we -- the issue is we are still in the midst of this data discovery and coming up with a finalized or a time line that, if you want to say, is cast in concrete or stone that's fixed. We're not satisfied with some of the components of the time line at this time. Okay.

Goals and objectives of the modeling. These were the

goals discussed with the epidemiologists when we first met as to what they needed for the epidemiologic study; arrival of contaminants at the well. And obviously, that also means concentration values or ranges, not just when they first arrived at the wells.

From the distribution side, the distribution of contaminants by housing location. We've sort of -- and housing location is taken to mean, like, Tarawa Terrace, Holcomb Boulevard; not necessarily House, you know, 2103. That's my interpretation, but as I said, the piping-system network does go down to the street level.

And it's always been our intent to address uncertainties. We understand their impact and the impact they can have, especially on interpreting results from the epidemiologic point of view and what sort of confidence. Just as an example, when we were doing our Dover Township work, the epidemiologist came back to us and asked, "Well, now that you've given us that House A receives 10 percent of the water, does that mean it's 10 percent plus or minus 50 percent, or is it 10 percent plus or minus 2 or 3 percent?" We had -- I don't know if it's luxury or opportunity there to tell them, "No. It's 10 percent plus or minus about 3 to 4 percent." We were able to reduce that out by running different scenarios for them.

Whether that proves -- or whether we have the ability

to do that here based on data, we're still looking into it. That's what we're looking for some of the input from this panel to tell us. And so -- and we've got the uncertainties on all sides: the groundwater analyses as well as the distribution side.

So to finish up, again, and this, I suppose, is more so for our public that's here but to go over a generalized approach. We've got our site, Camp Lejeune, here. And on the groundwater side, we're using the Modflow or one of its derivatives, which will become eventually coupled with a fate and transport analysis.

You have only been provided -- the panel -- with an advective part up until this point in time. But it's been our intent all along to go to the full-blown look at the dispersive issues as well and then, on the distribution side, an EPANET-type or its equivalent too. Again, we've used EPANET and its equivalent for our present-day analyses; actually to help us, guide us, in preparing some of the field studies.

And I believe that's all on the overview of the -- of the types of models. One point I wanted to make on the report that the panelists were given -- I'm calling it a report, and that's probably a misnomer. It's more probably a collection of data collection efforts and some background information.

And if we -- or if I implied that it was intended as a final or finished product, that was probably a miscommunication on my part. It was really meant to be a working document, hopefully presented in some intelligent form, that you could make sense out of it. So this is not an intent for you necessarily to review that document as a report but as the data contained in it.

And I believe that's it for the overview of the modeling. At this point, Dr. Johnson, we've got two options. I've got a brief overview on the groundwater and then leading into detailed discussions and analyses with Bob Faye. Or we had prepared some general responses to some of the premeeting comments. I didn't know if that was the opportunity -- if this was when you wanted me to just give an overview of those.

DR. JOHNSON: No.

MR. MASLIA: Okay.

DR. JOHNSON: I think it is, though, the time and opportunity to ask questions on what we've heard thus far. Yes.

DR. UBER: Morris, this might not be the best time to ask this question. So I don't -- I cannot speak myself authoritatively at all on chemical or biological processes affecting any of these contaminants, and so this question also maybe then goes to some of the panelists who can.

But do you know: Right now, do any of those potential chemical biological processes act in the distribution system? And if so, are their kinetics effective over residence time scales that are typical of distribution systems?

MR. MASLIA: I have to plead ignorance to that. I don't know if that's a question that Frank -- as far as biologic processes with respect to the epi part of things. I know that question came in other studies of biologic plausibility, the fact that you can make an association, say, between contamination of a water resource and an apparent disease. Is there, in fact, a biologic plausibility for that?

DR. BOVE: Oh, I didn't know -- I thought the question was more on processes.

MR. MASLIA: Oh, was it? Okay. I think I can --

DR. BOVE: Yeah; because I can answer that one.

DR. UBER: I think I can -- I was probably too wordy. I just want -- I'm basically asking: Does the team feel right now that for purposes of transport in the distribution system that they can model these contaminants as tracers?

MR. MASLIA: Based on what we've seen with the responses to the present-day system -- and that's all we have right now -- the answer is yes. In fact, we've made

some, I think, some interesting, if not eye-opening, observations based on how the present-day system is operating. And from what we have been told to date, that is a typical operation over the last 20 or 30 years with, of course, obviously, changes in hydraulic and infrastructure, removing treatment plants, starting up the Holcomb Boulevard treatment plant, things of that nature.

But based on the preliminary tests that we've done to date, we have been able to, I believe, do some acceptable -- not maybe final, but acceptable model simulations. And, in fact, it was the model simulations that led us -- and we'll get into this probably later this afternoon and tomorrow -- that led us to suggest to the utilities' folks at Lejeune that they, in fact, perhaps had some closed valves while we were doing it, relying on some -- and it turned out that that was correct.

So I believe -- to answer your question in a short manner, I believe the models will -- based on what we've seen to date will provide us the ability to provide some answers on that. As far as the level of variability or uncertainty, I think that's where we need to get back with the epidemiologists and really sit down and see what level they're willing to accept or can accept for their analysis. And that, I can't answer you at this point in time.

DR. UBER: Oh, okay.

DR. WALSKI: To give you a little answer to your question, Jim, on the processes, most of the things that happen to the VOCs in pipes don't really -- I mean, there's not much that can happen to them. I mean, in pipes, the only place where you could have much of a process affecting them is usually in tanks where you have a free water surface and they can volatize.

But when Ben and I did the work in Phoenix/Scottsdale, we looked at that, then went back to Henry's Law and looked at stuff like that. And we did -- you know, since you don't really -- it's hard to measure these kind of things, and there's not a lot of literature on Henry's Law in a perfectly still tank. Usually, if it's for stripping towers and stuff like that, you have a lot of literature data.

But going back and trying to reconstruct this, we estimated 97 percent of what went into a tank came out. Very little is really lost through the surface, and that's about the only process that you lose VOCs is through the surface of the tank.

So basically, assuming that it's -- what goes in the system goes to the tap is probably, you know, a reasonable assumption if there's not processes occurring. At least, we couldn't figure out any processes that would knock down

the concentration significantly.

DR. POMMERENK: Yeah. I have some supporting information on that. Because that question was asked by Camp Lejeune to us as their consultants, we looked into literature and tried to come up with a rough estimate of would there be any removal within the treatment plant. And since, you know, we had to review all of the drawings of the existing plants, we knew the surface areas that are available. We made certain assumptions: You know, is the water quiescent in that tank, or, you know, is there any agitation anywhere?

In all the tanks that we looked in -- and some of the tanks are newer. There's more surface area available today than there used to be early in the seventies. But removal due to volatization was negligible. I mean, it was less than a tenth of percent. The only location where there would be some removal was in the spiractors that were operated in all these Hadnot Point, Holcomb Boulevard, and Tarawa Terrace plants.

And even there, there was a certain uncertainty, depending on they had conditions downstream you would get some agitation at the effluent pipe. So although we said it's probably negligible, and I agree with Tom's number here. At 90 percent, what's going in is coming out on the other end.

One thing that had to be -- we were not able to address. I believe the Hadnot Point plant used to have a carbon dioxide contact basin. We could not find out when this contact basin was operated because, obviously, that process would agitate the water significantly. It was also open to the atmosphere. It was not in a closed building. And there could have been some significant removal, but we were not able to be certain when this -- they ceased the operation of that unit at Hadnot Point a long time ago. And even some of the older operators that we talked to were not able to tell us when that was. But, again, you know, what Tom said is probably accurate, that you can probably use PCE and TCE as a tracer distribution system.

DR. WALSKI: Which leads to the question, though, on the measurements we have. We have only a handful of measurements of VOCs in the system. Were these taken before treatment or after treatment? When were they taken?

MR. MASLIA: There are some -- from the health assessment, there's some tap samples. So that obviously would be after treatment. We've got some groundwater wells with PCE and PCE measurements, so that's obviously before treatment.

DR. CLARK: But there's a third class that's on the

schedule that says water-distribution system.

DR. JOHNSON: Step up to the microphone, please.

DR. CLARK: I'm sorry. The time line also has water-distribution systems from neither tap nor well. And that's what, I think, the question is.

MR. MASLIA: It's somewhere -- tap is at the household.

DR. CLARK: No. Let me quote from it. It says, "water-distribution system tested."

MR. MASLIA: Right.

DR. CLARK: Was that -- at which side of the distribution system? I mean, at the tap?

MR. MASLIA: Oh, I see what you're saying.

MR. FAYE: I think that was on the treatment side.

COURT REPORTER: Excuse me. I can't hear you.

MR. FAYE: I believe it was on the treatment side.

DR. CLARK: Post-treatment.

MR. MASLIA: Post-treatment; post-treatment side.

DR. POMMERENK: Can I add to that? Thank you. As far as I'm aware of -- and you, Morris, you probably remember that too. The contamination of the drinking water was first discovered -- there was -- a portion of it was discovered in the early eighties when the -- after the promulgation of the THM rule, the trihalomethane rule. So these samples were taken in the distributions system at

consumers' taps, and I think in the course of the analysis, the laboratory that analyzed had problems resolving the peaks from, you know, from the THM compounds because I believe TCE or PCE was masking those other peaks on their chromatograms. So these early data may have been actually tap samples in the distribution system.

MR. MASLIA: Yes. We've actually got documents with the lab notation on there, specifically addressing that particular issue.

DR. JOHNSON: I have a question. With regard to the models, you indicated, I think, that they're both EPA models?

MR. MASLIA: No. No, sir. Modflow was originally developed in the middle to late eighties -- correct me, Lenny, if I'm wrong -- by the U.S. Geological Survey. It's a public-domain model. And now, of course, there are any number of proprietary codes that use it as the engine, more or less, with the data sets. Basically, if they say they're Modflow compatible, then you can run them with a plain vanilla code, which is publicly available from the USGS Web site, and we have done that.

EPANET is the same issue. That was developed by -- can I say this? -- your shop, Bob Clark's shop, when he was at EPA, by Lou Rossman. We've worked with it from Dover Township days, and again, a lot of the commercial

codes for the water-distribution models use the EPANET engine. We are actually using both a commercial or proprietary code and EPANET. Some of the commercial codes, as they do have nicer bells and whistles on the front-end to make data input a little easier and things like that. So there are two publicly available model codes that have been vigorously and publicly tested.

DR. JOHNSON: What do we know about their validity?

MR. MASLIA: There -- we're convinced of their validity. There's documentation. In fact, EPA has a documentation ad for specific problems to test for Modflow. And that's, again, available on the EPA Web site, that if you want to -- if you make a modification, if you will -- we have not made any modifications to the models, by the way.

But if you do and you want to test its verification or validity, then you can run those sets of problems. EPANET 2 obviously is a second-generation version of EPA, and it has gone through robust testing. And most of the commercial codes, again, will carry the -- EPANET has a set of problems that you can test your adaptation of it against those benchmark -- if you want to call it those benchmark problems.

DR. JOHNSON: Okay. Thank you. Why don't you continue with the other material, please.

MR. MASLIA: Okay. Thank you. At this point, what I want to do is give a very brief overview, more of a generalized overview, of this morning's -- the rest of this morning's session will be on groundwater. And then throw it over to Bob Faye to really address step-by-step technical issues.

So, Claudia, if you'll get the groundwater slide -- groundwater overview. Okay. There you go. Is that the first slide? No. I need -- back up one. Okay; one more. Okay. I've probably got them X'd out. Okay. I'll make it short and sweet then. Okay. Okay. There you go.

Sources of contamination, we've -- as we spoke about Hadnot Point being the first one leaking underground-storage tanks and spills and other waste disposal and then Tarawa Terrace, which is the dry-cleaning source. And that's really why in discussions with Bob Faye and myself and with some input from the epidemiologic side is where should we attack first.

In other words, we were more sure or more positive of Tarawa Terrace being as close to a single source as possible, an identifiable source. And so we decided from a project-management standpoint as well as initial results to show the applicability of what we were doing to go after Tarawa Terrace. So -- and that just gives you the dates. And the Well 26, which you'll probably hear a lot

about and it is on our time chronology, is about 900 feet from the dry cleaners. And that was the well -- one of the wells that became contaminated at Tarawa Terrace.

And so the approach to modeling groundwater was to assess Tarawa Terrace as a single source and a known location, known location for the source and to develop a geohydrologic framework. There have been some previous work done -- Bob Faye will get into the details of that -- both from the U.S. Geological Survey in the middle to late eighties being on site at Camp Lejeune as well as some private consulting firms doing some work; construct the three-dimensional Modflow model; calibrate the model for study state or predevelopment; and then look at transient conditions; and then conduct fate and transport. As of today, we have done all but -- with Tarawa Terrace -- except the fate part. We've done the advective transport.

And that's really all -- I just wanted to give a complete overview from the groundwater side to any members of the public who are here or who want to see the big picture. So that's the big picture on the groundwater side. And at this point, again, I'd like to introduce Bob Faye, who will give you the details of our groundwater-modeling analyses.

DR. JOHNSON: Any questions to Mr. Maslia with regard to the groundwater presentation?

DR. POMMERENK: I have one question.

MR. MASLIA: Oh, sure.

DR. POMMERENK: Morris, don't quote me on this. I don't remember quite -- in one of the public health assessments, I seem to remember there was another dry-cleaning business to the east of ABC. Can you just briefly state why this is not included in your talk?

MR. FAYE: Yeah. Is this on? Peter, I can address that. The initial study that was done in 1985 by Shiver, I think it's called Globa-something or other --

MR. ENSMINGER: Globarama.

MR. FAYE: Globarama; right; Globarama Dry Cleaning. The initial study that was done by NCDEM by Shiver in 1985, he looked at that -- at that facility in detail and decided that not only did their operations -- it was a closed operation, apparently, where they completely recycled their waste and handled their waste in a responsible way by hiring a waste management -- a concern to move the waste away from the site.

Also, there were groundwater samples taken near the site, as I recall, and it showed that there was no real opportunity at that site for groundwater contamination. For example, I think the observation well that they drilled right in front of the ABC facility, the concentration in September of '85 was about 12,000

micrograms per liter of PCE. And the contamination at the Globarama facility was minimal, was no comparison, if any. Did that answer your question?

DR. POMMERENK: Yes.

MR. FAYE: Was that -- okay. And that has been described and discussed in detail, not only in Shivers' report, but also in the EPA Operable Unit 1 and Operable Unit 2 reports that Weston --

DR. POMMERENK: Okay.

MR. FAYE: -- the Weston folks put together back in the early nineties.

DR. POMMERENK: Thank you.

DR. JOHNSON: Okay. Any other questions?

MR. FAYE: Okay. My name is Bob Faye. I'm a contract employee with the Eastern Research Group. And as Morris said, my responsibilities for the most part have been to construct and calibrate the groundwater-flow model to date.

Dr. Johnson, am I allowed to suggest that if the panel members have questions that they could just freely interrupt me at any time?

DR. JOHNSON: Oh, absolutely.

MR. FAYE: Okay; great. Please do.

DR. JOHNSON: About how long is your presentation?

MR. FAYE: I think probably -- well, depending on

questions, to complete the framework and the contaminant description as well as the flow-model description, probably on the order of 90 minutes or so.

COURT REPORTER: I'm going to need to take a computer break before then.

DR. JOHNSON: 90 minutes?

MR. FAYE: 90; as in 80, 90, 100.

DR. JOHNSON: Morris, we have a 10:30 panel discussion and answers to questions. This appears -- a 90-minute presentation would appear to be a serious overlap.

MR. MASLIA: Yes. Part of the answer to the question is we were going to direct feedback.

COURT REPORTER: Excuse me. Please get a microphone.

MR. MASLIA: Our intent was, I guess, with direct feedback during Bob's presentation, to start addressing some of those questions and perhaps hopefully -- not eliminate them, but have some discussion on specific -- those specific questions. Unless -- and the other suggestion -- not that that shortens the length, but I didn't know if you wanted to take the 15-minute break now and go through the entire presentation and go forth, rather than breaking it up for the scheduled break.

DR. JOHNSON: What does the panel wish to do? Take a break now?

(Audible responses)

DR. JOHNSON: Okay. We'll take about a 15-minute break and --

MR. FAYE: How do we resolve this, Dr. Johnson? Do you want me to just describe the groundwater-modeling effort? What does the panel -- well, I'm happy to accommodate whatever the wishes are or try to accommodate.

DR. JOHNSON: What I heard Mr. Maslia say that the idea here is to have the panel address some of the, what I call, the eight questions that the agency has put forth on groundwater and to try to integrate those into your presentation. And that leads to them asking questions during your presentation, and that seems to me to be quite a good process. So does that answer your question?

MR. FAYE: Right. Well, I'll just -- then I'll just continue with Plan A, and if somewhere in the interim we need to switch, we'll go to Plan B and Plan C.

DR. JOHNSON: Okay. I will say that 11:45 we're out of here as a stampede toward the lunch. So why don't we take a 15-minute break? Be back at 10:30, please.

(Whereupon, a recess of approximately 17 minutes was taken.)

DR. JOHNSON: Okay. Let's resume.

Let me suggest to the panel that you ask questions during Mr. Faye's presentation, and I think it would be

useful if you could relate some of your questions to the questions that have been provided by ATSDR that pertain to groundwater. And specifically, these are some eight questions that were provided to you in advance.

I know you also provided premeeting comments, and at some point, Mr. Maslia is going to provide kind of an overarching response to that. But feel free to blend in your premeeting questions and comments during the presentation here by Mr. Faye.

We will continue the groundwater discussion after lunch to some degree, to the point where we feel satisfied with it. And if we finish a bit early, then I'm going to push up the water-distribution systems questions to later in the day.

So I need, also, as a matter of courtesy and respect to introduce Dr. LaBolle. Would you introduce yourself, your affiliation, and I asked each of the other panelists to give kind of an initial reaction to the materials that you received.

DR. LABOLLE: Yes. I'm Dr. LaBolle from University of California, Davis, department of hydrologic sciences. And my initial reaction: I was quite pleased with the level of detail and work that's being done with the distribution system. My expertise is in groundwater, but I have some experience with distribution-system modeling,

in particular, models that are similar in construction with this groundwater linkage to the distribution-system model with the fate and transport involved as well.

And my greater concern is with the variability and uncertainty in the groundwater system, and I'll be posing some questions with regards to that.

DR. JOHNSON: We look forward to those questions.

DR. LABOLLE: Thank you.

DR. JOHNSON: And welcome to the panel. Okay.

MR. FAYE: You ready?

DR. JOHNSON: Yes.

MR. FAYE: Okay. Just to start out, I want to clarify one thing. You may hear me -- and I know in my -- in my papers that I wrote for the document, I use the term "Montford Point," but that's equivalent to Morris' Camp Johnson. Okay? So if I say -- if I slip and say "Montford Point," just think Camp Johnson.

The rest of the areas, he's already talked about: Tarawa Terrace area and the Holcomb Boulevard area. And those are the three areas that feature in the framework discussion. The Tarawa Terrace area features exclusively in the model discussion and in the description of contamination.

The purpose of the framework was to describe and quantify the geometry, hydraulic characteristics, and

potentiometric levels of the aquifers and confining units at Tarawa Terrace and vicinity at a scale and level of detail suitable for application to groundwater flow and contaminant fate and transport models.

As far as data are concerned, these -- this is inclusive of the Camp Johnson area, Tarawa Terrace area, and the Holcomb Boulevard area. Elogs, that stands for electric logs. We have a -- we have a poster with the -- with several examples of electric logs for your benefit.

There's two parts to an electric log: the resistivity side, the spontaneous potential side. Both are important and useful in terms of defining the various layers that we -- that we're dealing with in terms of the framework.

There were 100 boring logs that were available to us from a variety of sources. There were -- there are two reports that address -- or three reports, actually, that address the contamination relative to ABC One-Hour Cleaners. There were -- and then -- many, many boring logs associated with those reports. There's also a large number of boring logs associated with RI/FS investigations that are ongoing in the Tarawa Terrace area.

Claudia, could you move back to the previous slide; and the next one, please.

These boring logs, unfortunately, are not spatially well distributed in the study area. The boring logs

almost exclusively refer to -- I'm sorry, almost exclusively refer to RI/FS studies that are ongoing in this very southern part of Tarawa Terrace and, of course, in this northern area, just north and south of Lejeune Boulevard, between ABC One-Hour Cleaners and Supply Wells TT-26 and TT-25. And we'll be talking about those in just a second.

That's a picture of a typical Elog that we have to deal with. The spontaneous potential curve, which is the left-hand -- the left-hand curve, is not very useful at Camp Lejeune because it's a -- it's, more or less, an industrial area. You've got a lot of ground currents, a lot of current loss in the subsurface, which causes reversals of the spontaneous potential curve.

Also, you have cycling going on; 60 cycles per second in the subsurface. You have bleeding out of the -- out of the electrical conduits that are buried, which also confuse the resistivity side. But for the most part, all of these analyses were based on areas or zones of low and high resistivity and not related back to the spontaneous potential.

This is typical of a boring log, one of the hundred. I think this extends to a depth of about 20 feet or less. Just a couple of points: This is the detail. These are mostly logs from augering, hollow-stem augering. So you

have a lot of smearing in the lithologic descriptions going on, probably plus or minus half of a logger stem, which is typically 5 feet. So any of these depths that you identify as perhaps a top of an aquifer or a top of a confining unit have to be identified in that context, that we're looking at something that might be accurate to only within plus or minus several feet.

A number of the boring logs were created using split-spoon samples at different intervals. Those, of course, are accurate to the identified depth, and they're very accurate. Many of the logs -- many of the boring logs in the Tarawa Terrace area, the northern part of Tarawa Terrace area, the ABC Cleaners' area, identified a feature called "running sands." And this -- this was -- shows universally as the top of the Tarawa Terrace or the -- top of the upper Castle Hayne aquifer. And I can tell you -- I can explain the rationale for that at some time later.

This is typical of the drillers' logs that we had available to us. In fact, that's quite a good one compared to many. That's the kind of detail that we looked at; the lithologic descriptions. Most of the time, I use the drillers' logs just to identify the occurrence of what was called limestone or Copena.

There was a major, major problem in locating accurately the various points of well-data collection, of

monitoring wells, particularly for the many RI/FS studies that were -- that were conducted there relative ABC Cleaners and these other places. That was the 100 boring logs that we -- that I discussed.

Virtually, the reports did not -- we used the state plain coordinate system for North Carolina in 1983, 9-AD -- NAD. Virtually, none of the reports use that system, so we had to convert the coordinates that were available to us. Many of the coordinates in the report -- in some of the reports were not correct. They were -- even on their own system -- whatever arbitrary system they devised.

So basically, what we did was just go back to the old-fashioned way of measuring distances on the maps that were provided. And we were able to identify -- you'll see this -- the little building there, TT-47. We would take intersections of roads or identified buildings or whatever and use that as the -- we would find the state plain coordinates for those places and then extrapolate those coordinates to the rest of the map, basically just using hand measurements. So you need to keep that in mind as well as you think about the accuracy of the location data.

Finally, the end product of the geohydrologic framework analysis was the development of 11 or 12 -- actually 11 -- 11 units as part of the framework, aquifers

and confining units. Now, as far as the Tarawa Terrace area is concerned, the Brewster Boulevard aquifer and the Brewster Boulevard confining unit do not occur at Tarawa Terrace except perhaps as a -- just a thin mantle of sediments at the surface that are -- that are smeared with every -- with everything else and really not of use to be identified or not even -- they're unsaturated almost always. And they're not dealt with in the Tarawa Terrace area.

I might say two things about the correlation effort. The U.S. Geological Survey produced two reports exclusive to the Marine Corps base Camp Lejeune back in the late eighties. And both of these reports had long, detailed sections, using various Elogs and drillers' logs and whatever; published these sections.

They identified a number of units that they would track on these sections across almost the whole entire base from well to well or Elog to Elog. And essentially, below the Tarawa Terrace confining unit, our geohydrologic framework conforms very, very closely with a few exceptions here and there to the framework analysis that was -- that was performed by the U.S. Geological Survey.

Relative to the Tarawa Terrace aquifer, Tarawa Terrace confining unit, and the Brewster Boulevard and Brewster Boulevard confining unit, we sort of did that on

our own. And some of our results at certain places differ from the USGS interpretations regarding these two aquifers.

One thing that I -- one thing that I like to do when I develop a conceptual framework like this is to constrain my results using chronostratigraphic boundaries. That -- that would be like actual geologic unit times. Unfortunately, for this particular study, that type of information was very limited. But I did use the distribution of the top of the Castle Hayne formation, which I identified with the top of what I call the local confining unit. That is the top of the Eocene. And I identified also the top of the Beaufort confining unit, which the US -- USGS has identified as the top of Paleocene.

And what you do essentially is you look at the -- you look at the strike, the distribution of those particular units. That helps you to understand the depositional cycles that occurred, that you're trying to identify as aquifers or confining units. That helps you identify the depositional cycles that occurred within that particular time frame.

And that's important because if you're just correlating a clay to a clay from Well A to Well B, you could just very easily be missing a facies change;

whereas, if you can -- if you can correlate it as well with a chronostratigraphic line, you have some confidence that you're looking at a spatially continuous unit in the subsurface. And we did that. We did that as well as we could with the limited amount of chronostratigraphic information that we had.

And then there's just a whole series of maps that you have in your report. This is the top of the upper Castle Hayne aquifer. This is one of the time units that I just spoke about that I used to sort of keep me on track in terms of the spatial distribution; orientation to the north, south, east, or west; dip and strike that I would apply to units below that and also actually to the River Bend unit, which was above it. And there's the thickness of the upper Castle Hayne.

Almost all of these surfaces that I've identified as either the top of a confining unit or the top of an aquifer are erosional surfaces. Okay? So you would expect some degree of irregularity in the -- in the altitudes at the top as well in the thickness and formation. And I wasn't disappointed at all in that regard.

Another feature of the geohydrologic framework analysis was the -- was the computation, the analysis of aquifer-test data. We probably had -- between Camp

Johnson, Tarawa Terrace, and Holcomb Boulevard areas, we probably had close to five dozen aquifer tests. Almost all of these invariably were single well tests, and almost all of the single well tests were step-drawdown tests.

And what I used -- what I used in for almost all these analysis is the public domain U.S. Geological Survey aquifer test analyses worksheets, Excel worksheets. And the real advantage to those is one -- it has one of the best approaches and methods to analyzing step-drawdown data, which was the majority of my data. And this is just an example of one of the output sheets.

Now, there was a question -- somebody addressed the notion of preferential zones of high permeability within the -- within the various units -- within the various identified aquifer units. We had no opportunity to do that except in the context of the resistivity curves on the electric logs. We could identify, perhaps, where there may have been a relatively thin lensoidal clay within the overall sand that we identified as an aquifer. But there was no way to, in my opinion -- and if folks here on the panel have some suggestions, I'd be happy to hear it. But we did attempt to quantify. That was just strictly a -- that would be strictly just a qualitative analysis, and frankly, it didn't really occur that much.

Another feature of the -- of the geohydrologic

framework analysis was the spatial mapping of the horizontal hydraulic-conductivity data that we determined from the aquifer-test analyses. That's the -- such as it is, that's the spatial distribution of the data for wells that were open to the upper and middle Castle Hayne aquifers.

The last thing that we did with respect to the geohydrologic-framework analysis was try to -- try to create a picture of what the prepumping conditions or predevelopment conditions were in the -- in our areas of interest, which were Camp Johnson, Tarawa Terrace, and the Holcomb Boulevard area.

And the way we did this was to identify the -- at a particular well site -- excuse me, was to identify the earliest measurement that we had available to us in terms of a water level. And in particular, in the Holcomb Boulevard area, we were quite fortunate to have a lot of -- quite a good number of measurements that were -- that were obtained in the early 1940s when the first supply wells were drilled.

We either chose the earliest measurement at a site, or we took the highest measurement at a site. If we were fortunate enough in a very few cases to actually have multiple measurements, multiple water level measurements, at a site, it was -- I could probably count those on one

hand -- but except for the Tarawa Terrace supply wells. But we chose either the highest measurement or the earliest measurement, and we just spatially plotted those data. And the data almost completely refer to either the upper Castle Hayne aquifer or the -- and the middle Castle Hayne aquifer.

But the notion here was just to look at possible boundaries that might be indicated as a predevelopment condition as well as flow directions. And what we find is that -- what we find is, as expected, Northeast Creek is an obvious boundary at least as far as these aquifers where the water-level information was obtained is concerned. And we have flow directions in Tarawa Terrace, generally either east or south, toward Northeast Creek. And in the Holcomb Boulevard area, we have flow directions north, west, and somewhat northwest, toward Northeast Creek.

And what this tells us is that, at least as far as those upper four aquifers or so are concerned, Northeast Creek is probably a major flow boundary. What this does as well -- and we have one site just north of Wallace Creek, I believe, right in this area here where there is a -- there's one -- there's a cluster site.

There's a series of wells there that are open to several of the units that we identified as aquifers here.

In particular, there's a well open to the Tarawa Terrace aquifer and intermediate to the middle Castle Hayne and also to the lower Castle Hayne. And that's just north of Wallace Creek.

And interestingly there, there's only about a 2-foot head difference between the head in the lower Castle Hayne aquifer and in the -- and the Tarawa Terrace aquifer. And I know that's not a lot to go on, but, as far as the conceptual model, which we'll talk in terms -- we'll talk in a minute about in terms of the model.

The conceptual model that we developed for guiding our approach to the flow-model analysis is that the predevelopment of potentiometric surfaces in all of the aquifers were relatively similar, in fact, very highly similar, so that, as far as the River Bend unit and as far as the lower Castle Hayne aquifer, the flow directions and the distribution of head in the aquifers was highly similar. And that tells us that Northeast Creek, indeed, would have been -- well, it is a boundary for flow for all of the aquifers that we're dealing with.

And I'll just take a minute to explain the reasoning there. You have groundwater flow -- pick your aquifer: River Bend unit or Tarawa Terrace aquifer, whatever. You have groundwater flow heading down gradient toward Northeast Creek from Tarawa Terrace, and that's heading

generally south. You have groundwater flow heading east and north in the Holcomb Boulevard -- Holcomb Boulevard area toward Northeast Creek. Well, this flow has to meet in the middle somewhere at Northeast Creek. And at that point, you have vertical upward flow in the vicinity of the creek. And that was the rationale behind us selecting the midline of Northeast Creek -- the midchannel line as a flow boundary -- as a no-flow boundary for the groundwater-flow model.

Also, in these USGS reports that I mentioned earlier, there were some seismic studies that were conducted in the water of New River and Northeast Creek, right around this Paradise Point area. And what they -- what they discovered was that there were buried subsurface channels that were relic -- relic river channels that were now under water. And probably, these relic channels manifest themselves inland as well as zones of relatively high hydraulic conductivity.

But our -- the distribution, the spatial distribution, of our well data are not sufficient that we can actually identify what that old relic channel would have -- where it is and what it would have been. And that may be one of the reasons that we have some irregularities in our -- in our surface well data as well as in our thickness data and also in our hydraulic-conductivity data

where, just by chance, one of these wells may have been developed in part or all of an old river channel, which would have been now filled with sand and would be an area of relatively high hydraulic conductivity.

DR. KONIKOW: Bob, what was the -- what's the rationale for the northern limit of your contouring on all of these maps?

MR. FAYE: We have a -- we have digital elevation models, Lenny, of this larger area. Let me show you. We have digital elevation models of this whole large area here. Actually, I think, probably of most of Camp Lejeune, but I was just looking at this. And that is interpolated to 2-foot contour intervals. And so using the -- using that, I identified the divide that ended up as the northern boundary, the no-flow boundary, in the groundwater flow-model.

I identified that as a hydraulic divide that generally sweeps up like this and down like that, and that's a hydraulic -- that's a topographic divide that is translated to a hydraulic divide in the groundwater-flow model. As I said -- and, of course, those are 2-foot contour intervals on the DEM, and they're interpolated as well. But that's the best information that we have. Okay?

DR. KONIKOW: Okay. I was looking at the topo maps.

It looked like there were -- I mean, I couldn't see the divide that close.

MR. FAYE: No, you can't. You can't, Lenny. There's a -- I can show you later, when we get into this, a much larger map specifically of the Tarawa Terrace area. There's -- you might have noticed that just north of this road that runs parallel to Lejeune Boulevard, there's a -- there is a closed 35-foot contour right north of that road, and that sits on that -- that sits on that divide. That is mapped on the topographic map. And that coincides with -- that coincides with that -- with the divide, as recognized on the digital-elevation models.

DR. LABOLLE: Are you going to -- this is Eric LaBolle here. Are you going to get more into the simulation of the predevelopment heads?

MR. FAYE: Yeah.

DR. LABOLLE: Okay.

MR. FAYE: Yes. This is just the framework.

DR. LABOLLE: Okay.

MR. FAYE: It'll show up very well, Morris, in the next couple of slides. Okay. Claudia, let's go to the description of the PCE contamination at Tarawa Terrace. There we go.

Okay. The next major area of responsibility that I had was a description of just what is this PCE

contamination at Tarawa Terrace. Where is it relative to the source area? Where is it relative to the supply wells? How deep within the subsurface does it go? What are the quantities; i.e., concentrations in the water? What are the concentrations in the unsaturated materials? So let's try to address that.

The purpose of the study, again, for the record, is describe the occurrence and distribution of PCE and related contaminants within the Tarawa Terrace and upper Castle Hayne aquifers at and in the vicinity of Tarawa Terrace housing area, Marine Corps base, Camp Lejeune.

And a number of comments in the premeeting notes were related to degradation products of PCE, and, yes, to the best of our ability -- and we're severely limited by the data here. But to the best of our ability, we did -- we addressed trichloroethylene, which is the immediate degradation product of PCE, as well as dichloroethylene, the immediate degradation product of TCE, trichloroethylene. We addressed all of that as well as we could, but the data are very limited; very, very limited.

Okay. Here's a map. Maybe we can see that 35-foot contour. There you go. Can you go back, Claudia. There you go, Lenny; right here.

COURT REPORTER: Please get on your microphone.

MR. FAYE: Thank you. There you go, Lenny. That's

that -- that's the contour I was talking about right there. And that's right on the line as shown on the DEM and comes down to -- it splits the difference between one of these two little tributaries right in here, I think. I think it's that one. It could be that one.

DR. KONIKOW: You also have a 35-foot contour a little further north.

MR. FAYE: Yeah; right; right. And there are differences between the DEM and the topo map, as you would expect. Actually, some of that is fairly significant, substantial. The differences are somewhat substantial. I can't recall now exactly what -- what's going on up here with respect to the DEM. But I looked for the major divide between here and there, northeast and southwest, and selected it.

Now, that may not be the -- from a groundwater modeling point of view, that may -- and particularly a fate and transport point of view, that may not be the best -- the best boundary. But, really, if we try to extend that north beyond the hydraulic divide, then we're stuck with a general head boundary, probably, for all of the units that we're modeling. And it just seems to me that would introduce more uncertainty into the -- into the analysis than selecting the hydraulic divide as the topographic divide. But let's -- let's -- go ahead.

DR. KONIKOW: I'm not convinced of that. Plus another problem is that during pumping conditions that predevelopment divide -- if that's really where it is and I'm not convinced of that either -- that the divide is going to migrate under pumping conditions.

MR. FAYE: It will. I don't think -- I don't think the -- at least as far as -- we don't really know. We have no data at all, field data, relative to -- relative to any kind of notion of radius of influence of the supply wells; no data whatsoever, so --

DR. KONIKOW: That could be computed --

MR. FAYE: We did.

DR. KONIKOW: -- more accurately than a lot of the other things.

MR. FAYE: Yeah. We looked at that. It just depends on where you want to go with the minimum drawdown out at some radius that you're looking at, whether it's .01 feet or .1 feet or something like that. I mean, that bounces your radius of influence all over the place. And right now, I'm fairly comfortable with the notion of using that hydraulic divide not only as far as the predevelopment situation is concerned, but as far as the transient.

But I would certainly welcome any kind of qualification or criticisms, comments of that notion. I mean, we're open to all that, absolutely. But I wanted

you just to be aware of my reasoning, you know, as far as the decision was concerned to identify it as such.

DR. JOHNSON: David, you have a comment?

DR. DOUGHERTY: No. I think we can proceed.

DR. LABOLLE: Well, I have a question here, actually, regarding the -- not the hydraulic divide. But since we're on the subject of boundary conditions here --

MR. FAYE: If we could -- if we could just be patient just for a minute and let me get through the contamination, then we'll be into the heart of the groundwater model. Okay?

DR. LABOLLE: Okay.

MR. FAYE: And that might be the best place to discuss that. I didn't mean to --

DR. LABOLLE: No. That's just fine.

MR. FAYE: Okay.

DR. LABOLLE: That's probably an appropriate opportunity.

MR. FAYE: This slide just identifies all of the Tarawa Terrace supply wells that we know of. There actually may be several more that we don't have knowledge of, but this is all of them from the beginning of time, which is -- it'd be about 1952 up to the time in 1987 when all the wells were shut down. And, of course -- and, of course, some of these were taken out of service long

before 1987. And as part of our plans, we have identified various data reports that we plan to produce.

And, of course, in the final report, there will be data reports, and all of these data will be tabulated and identified in terms of well-construction information, when the wells were placed in service and removed from services, et cetera, et cetera. We do have that information for most of these wells. We have good information regarding that, not only from our own data discovery, but the AH people have been very forthcoming and helpful in that regard.

Claudia, I'm going to go one more slide, just to orient myself here; just a second.

All right. Let me talk a little bit -- and I think this is very important to understand. Let me -- even though we're a little pressed for time. But let me talk a little bit about the contaminant data collection at ABC Cleaners and vicinity as well as the Tarawa Terrace supply wells that were affected in terms of timing, in terms of concentrations, in terms of quality of information.

What this slide represents is a summary of several series of data that were collected between 1991 and 1993. And I went into some detail in this in the report, but I want to say it here as well for the record.

The vast majority of these data that you see

portrayed here -- summarized here actually -- relate to DPT data, hydrocone data, direct push technologies. We all familiar with that? You know what I'm talking about? Okay. There were probably like 40-some -- almost 50 of these DPT points where data were collected at -- in an upper zone, generally between about 15 and 25 feet, and at the same site in a lower zone, generally between 35 and 45 feet.

And what you see here is a -- is the -- if it happens to be one of those dual sites, this is the highest concentration that occurred at that site, whether it was the upper shell or the lower shell, the upper zone or the lower zone. Several comments about those data: There was an analysis done from a field mass spec operation at the site when the DPT operation was ongoing, and there were results obtained from that.

The -- Weston, the folks that conducted that site, also collected a number of duplicate samples and sent these off to a qualified laboratory for analysis. The end result of that was that there was very poor agreement between the laboratory analyses and the on-site analyses for a particular bore hole or whatever. So we have that particular problem. By the way, the points that were used to construct this map were all the laboratory analyses where they were available. Where they were not, we used

the field -- the field site data.

Several -- okay. Let's look at -- here's ABC Cleaners. A point that I'll make later in our advective transport analysis when I describe that -- and, again, I apologize. I'm talking about a model here. But it'll be clear in a minute. The Well TT-26 is right here, and at least as far as our model is concerned now, under normal operation, the operation of TT-26 would capture every bit of the PCE that was introduced into the subsurface and into groundwater at ABC Cleaners.

But we have fairly large concentrations of PCE north and west of ABC Cleaners. And in addition, we have respectable concentrations of PCE south of -- south of the well here, TT-26. And this is near another supply well, TT-23. But as you can see, PCE values or concentrations values at this time, now 1991 to 1993 -- you have to remember this is four to five years after the Tarawa Terrace wells were shut down -- there's zero concentrations here. And these points I'm making now because they'll occur prominently in the discussion of the groundwater-flow model.

Okay. We had these data, as I mentioned, of the PCE concentrations and other contaminant concentrations that we could assign to an upper shell and a lower shell. So given that, we created -- is that it? I'm going to go for

this now. We created a map. Thanks, Claudia.

We created an average or a midconcentration map, using the aerial distribution, the spatial distribution from the upper shell and from the lower shell. With that midconcentration shell, we also computed the volume of aquifer material between the two shells. And in doing that, the DPT data we actually used the depth they identified. If it happened to be a well, we used the midpoint of the screen interval to put a limit on the volume -- on the depth.

We computed the area-weighted PCE concentration between the average shell-concentration contours. That, in a sense because it's the midconcentration shell, is the volume-weighted PCE concentration. Once we had that, we multiplied that by the volume adjusted by effective porosity. And we ended up with a PCE mass of about 2500 pounds between those two shells or 185 gallons of PCE. And this analyses, I think, is described in pretty good detail in the report.

DR. KONIKOW: Bob, why do you use effective porosity rather than total porosity?

MR. FAYE: Yeah. Well, if you recall, Lenny, there was a -- there was also a description in the report of the movement of the mass of concentration, the center of mass of the PCE concentration, from the doorstep of ABC

Cleaners, in '85, down to some point midway between ABC Cleaners and TT -- Well TT-26.

Well, the reasoning there was that that movement had to occur through connected interstices in the porous media. And where it ended up in 1991 to '93, the volume that that PCE was occupying was only connected interstices, not the -- not the total interstices in the porous media. So as a consequence, we used effective porosity.

DR. KONIKOW: Well, you know, I think if you have the contaminant in the connected interstices, it's going to be in the -- I don't see any way to have uncontaminated water adjacent to it in the disconnected pores, even if there are. And I find it hard to believe there are disconnected pores there. You used a specific yield value of 20 percent, I believe.

MR. FAYE: In Layer 1 in the Tarawa Terrace aquifer, that's right. The rest of -- the rest of the layers -- like, the River Bend unit is 15 percent, and that's where the vast majority of the contaminant is. Now, we don't have any measurements of effective porosity. We don't have any point measurements.

Two of the studies that -- the Weston study and, I believe, the Bragg's report as well, used effective porosity depending on the on the unit they were -- of 15

percent and 10 percent. And I kind of qualitatively looked at the lithologies and assigned a slightly higher effective porosity to the Tarawa Terrace aquifer.

It looked to me like that was a cleaner, sandier unit. The 15 percent, I accepted for the River Bend unit. And I really couldn't see a whole lot of difference in the lithologies between that unit and the other aquifer, so I assigned a 15 percent effective porosity to the -- to the rest.

But the one point would be that, you know, this is just a preliminary calibration. Okay? We really haven't -- we really haven't had an opportunity to do all of the tests and provide all of the simulation results that we want to, so...

DR. KONIKOW: It's in my comments. But I looked at -- there was one part in your report where you say the center of mass migrated at about .3 feet per day.

MR. FAYE: That would have been an average, yeah, given the distance.

DR. KONIKOW: But if you used that information, together with the other information, you would estimate an effective porosity of about 28 percent.

MR. FAYE: At a retardation factor of one.

DR. KONIKOW: If there's no retardation.

MR. FAYE: Yeah. And if there is retardation, which

I do believe there is, your effective porosity then would -- to maintain that same average velocity, your effective porosity would have to decrease from that number. And really, I think the way to address that, Lenny, is to, you know, take your comment and the notion of the analysis, which I thought was really on target, and just do a range of computations and look at -- look at the various alternatives. And that's what -- we'll definitely do that.

DR. DOUGHERTY: Is there information from the split-spoon samples that you referred to earlier that gives total porosities that would provide some boundary information on where we are with respect to those?

MR. FAYE: You know, I won't say no. If there -- if there are, they would be -- there would be very, very few. And they would be probably only related to the Tarawa Terrace aquifer or the River Bend unit. Okay?

DR. DOUGHERTY: Okay.

MR. FAYE: Okay.

DR. LABOLLE: Can you define how you're using effective porosity in this context?

MR. FAYE: Only in terms of the advective transport.

DR. LABOLLE: That's not what I mean. I mean, are we talking about effective porosity at the pore scale, or are we talking about some macroscopic effective porosity to

scale the velocities in the contaminant transport model?

MR. FAYE: Yeah. Well, the correct answer to that is yes (laughter). And I'm not trying to be a smart-ass. I'm just saying that, you know, we're sort of stuck with -- when you do the advective transport modeling, obviously, it's a macro-scale condition. Okay? But if we have any data at all, it would be -- it would be data only on a -- it would be like a laboratory test that you could probably relate to the pore scale itself. Conceptually, we're dealing with the pore-scale concept. Okay? But in practical application, it's a macro scale. Okay?

DR. LABOLLE: Okay.

MR. FAYE: And let me go back now. We'll look at some temporal -- are there any questions at all about the PCE mass? I want to make one other comment about that computation. Pankow and Cherry, not only in their text but also in at least one journal article, they address this particular methodology. And they have some comments about the results.

One comment that they -- that they make is the fact that that particular result of 185 gallons -- actually, they give several examples, like seven or ten examples in their work. It sort of fits midway into their -- into their volumes that they've computed for -- at various -- various places and various studies. Also, they make the

point that this is very likely just a very small percentage of the total PCE that's actually out there in residence in the aquifers themselves, and we believe that as well.

MR. MASLIA: Am I on here? I believe -- and Bob brought this to my attention -- there, either through some verbal information or a report that quantified that, they estimated that the ABC Cleaners were using approximately 100 gallons a month of PCE historically in their dry-cleaning process. So again, the 185 is an extremely small --

MR. FAYE: Yeah.

MR. MASLIA: -- percentage of what potentially could be out there.

MR. FAYE: Yeah. I hate to waste 60 seconds on an anecdote, but I am because it gives you a -- just clarifies the kind of things that we're dealing with. Wouldn't you believe that if someone is conducting an RI/FS investigation twice relative to ABC Cleaners that one of the things they would at least do would be to ask those folks how much PCE they're actually using during their operations or did use during their operations? No. Nowhere in the RI/FS reports, the detailed technical investigation reports, nowhere do you find any kind of reference at all as to what was happening at the source in

terms of PCE use.

The report Morris referred to is something I ran across fairly recently. It was a report from the National Oceanic and Atmospheric Administration, who were looking at the impact of this PCE loss into the groundwater on wildlife and wildlife habitat in Northeast Creek. And those folks actually had enough sense to go and talk to the ABC Cleaners and ask them, "How much PCE do you folks actually use a month in your operations?" And it turned out to be about 380 liters or 100 gallons a month.

MR. MASLIA: Dr. Johnson, there's a question from the public.

DR. JOHNSON: Please. Go ahead. State your name, please.

MR. ENSMINGER: (Off microphone) My name's Jerry Ensminger. I was a resident there.

COURT REPORTER: Can you state your name again, please.

MR. ENSMINGER: Yes. My name's Jerry Ensminger. I was a resident there. I lost my daughter to leukemia. When you're talking about historical data, and especially ABC Dry Cleaners, there are a lot of variables in that site that need to be considered. And one thing is the historical information: What took place between 1965 and 1970 which involved the Marine Corps and increased the

population of the Marine Corps almost two-fold, and that was Vietnam.

From 1965 to 1972, that was the heyday for dry cleaners in Jacksonville. Did anybody get the tax records from these people because PCE would have been an expense which would have shown how much they actually used? And knowing the amount of people -- every Marine that went in the Marine Corps east of the Mississippi River ended up at Camp Lejeune to go to their infantry training school at Camp Geiger.

These dry-cleaning services had trucks that went aboard base, collected these kids' uniforms at the chow halls in the morning and brought them back that night or the next morning. They picked them up. But every Marine east of the Mississippi went through Camp Lejeune. These people made a fortune during those years, and the PCE use was elevated. Thank you.

DR. JOHNSON: Thank you. Thank you for your comment; absolutely.

MR. FAYE: Claudia, could we go back a few slides to the -- there we go. Keep going and maybe one or two more; one more. All right.

These slides represent what we have at the wellheads in terms of contaminant concentration through time. Beginning in late '84 or early '85, these are our data

points that we have. This is Well TT-26. This is probably the main culprit in terms of providing PCE to the water-distribution system, far and away, probably. But you can see the poor distribution of data.

Now, enter -- let's go -- let me see what we have here. That was PCE. This is the daughter product, TCE. Virtually, the analyses are for the same time. And you can see there was -- you can make a pretty good case there that biodegradation of the PCE product was going on.

DR. JOHNSON: And what's the source of these data?

MR. FAYE: Who asked that?

MR. MASLIA: Dr. Johnson.

DR. JOHNSON: What's the source of the data?

MR. FAYE: Dr. Johnson, there are a variety of sources. Some of it came from LANTDIV, the Marine -- the Navy lab. Some of it came from EPA. Some of it came from the North Carolina EPA equivalent.

DR. DOUGHERTY: Do we have any information on sampling protocols?

MR. FAYE: Only in the -- only in the latter reports, the latter analyses, which would be in 1991. We think -- have to assume that if NCDEM, North Carolina Department of Environmental Management, did the analyses or the LANTDIV people did the analyses that it probably was a respectable representation of the protocols at that time. And they've

changed a lot. The protocols have changed a heck of a lot in the last 20 years, so...

DR. DOUGHERTY: Just to clarify, at that point in time, there were pumps still in these wells?

MR. FAYE: Oh, yeah. Yeah. The wells were actually abandoned formally; and that is, grouted up, pumps removed, everything like that in 1991.

MR. MASLIA: David, I have a document, again, just received. I hate to keep saying "just received," but you know the story. And, in fact, it lists many of the TT wells, and it will say "Well closed but pump still installed in the well," and TT-26, TT-23, and so on. And this is a nine -- I believe it's a '91. I believe I left it on the desk there; a '91, '92 report. It's handwritten notes. It's a document released by the Marine Corps to us. But it does indicate whether the well can be operated and whether it still has a pump or the well does not have a pump and can be operated.

MR. FAYE: You know, and that was a note from the -- from the folks at the facilities -- in charge of facilities at Camp Lejeune to the EPA contractor, who was inquiring whether or not these wells were sampleable. And almost immediately, as far as I can tell, after this contractor obtained those July 1991 analyses, those wells were history. They were grouted up. They were done.

Now, also, recently -- we keep referring to these recent revelations that we get. We have -- actually down to the -- down to report numbers, dates, sample numbers, the whole thing. We have information regarding monthly samples at Well TT-25, which was not -- which was actually right about here. And this -- in July of 1991, there was an indication that Well TT-25 was beginning to show contamination in its discharge.

And North Carolina DEM recommended that monthly samples at TT-25 be collected over the period April -- actually until the well was shut down. But the samples were collected from April of '86 to April of '87. And we're making major, major efforts now to obtain the results of those analyses. The Marine Corps doesn't seem to know anything about them. But we know -- we know the samples were collected. We know the analyses were made. We have sample numbers and report numbers. So we're trying to -- and that will fill in some of that, some of that gap.

Yeah. Also at the -- in the same documents, there were weekly samples taken on the downstream end of the Tarawa Terrace WTP at the same time, which would -- which would help Morris' efforts to -- and the network simulation efforts immensely. Again, we're trying to find those data. We know they exist, but no one seems to know

where.

MR. MASLIA: Let me just qualify. Those data were -- there was a panel in September or October, convened by the commandant of the Marine Corps, and it's a published report. It's on the Marine Corps. And in Appendix or Attachment K, they list some of those data. The issue that both Bob and I have with that is that the Marine Corps commandant's panel left out -- and I'm not sure why -- any qualifiers on the data and any of the nondetects based on their interpretation.

I have requested that, and there was a letter from the U.S. Navy to U.S. EPA Region IV, transmitting the data weekly for a various number of wells with these attachments. EPA doesn't have that -- the attachments, and apparently, my last communication with headquarters Marine is they're working on finding the attachments. But that would, again, supply us with what appears to be, on the surface, very needed information because it goes from, I believe, the first week in December of '84 through about '86.

DR. JOHNSON: Bob, if I could go back to your contamination --

MR. FAYE: Oh, yes, sir.

DR. JOHNSON: -- data. I didn't see any error bars for each of the data points. And is that not done for

this kind of data? If it were a tox study, you would expect to find it.

MR. FAYE: When you say "error bars," you're --

DR. JOHNSON: Standard errors, standard deviation; some sense of variability at each data point.

MR. FAYE: Well, at the very -- at the very most, Dr. Johnson, except for those supply wells that we have, that I showed you through time, the spatial maps like that at the very, very most, we have only two samples.

DR. JOHNSON: Okay.

MR. FAYE: And those are for different levels. Remember, I talked about the upper shell and the lower shell, and that's all we have there. There were -- we could do some sort of cursory analyses like that for the half a dozen samples that we have at a single site like -- but that's so dynamic, you've got biodegradation going on.

DR. JOHNSON: I understand.

MR. FAYE: I don't know what that would show.

DR. LABOLLE: How do you explain the region between the two plumes with the zero concentration? What's your interpretation of that?

MR. FAYE: That, I'll talk about in the model. Okay?

DR. LABOLLE: Yeah.

MR. FAYE: Yeah. That's after a lot of aspirin, believe me. Okay. We've got a few minutes left to talk

about the model. Let's get going. I'm not going deal with the introductory material. Let's do the purpose of study.

Construct and calibrate a groundwater-flow model sufficiently representative of the geohydrologic framework and groundwater-flow conditions at Tarawa Terrace and vicinity to support fate and transport simulations. You've already seen the well locations. You know what the aquifers are and confining units.

Let's describe the model grid very briefly: 270 columns, 200 rows. That's the complete model domain. That's the inactive and active areas, 24,000 active cells. All of the active domains are spatially equivalent. The cell dimensions are 50 feet by 50 feet.

There's nine layers, and they correspond exactly to the geometries of the aquifers and confining units that we've identified. Frenchman's Creek -- could we -- could we go back to that; Frenchman -- Frenchman's Creek is a -- sorry. Frenchman's Creek is a small drain in the western part of Tarawa Terrace, and that's -- that's accommodated in the model as a drain in Layer 1, which is the Tarawa Terrace aquifer.

Northeast Creek, the whole area -- sorry, Claudia. Northeast Creek, this -- the whole area down to the midchannel line, which is our no-flow boundary, is a

specified head boundary, zero altitude, in Layer 1. In the other -- in the other eight layers, it's just an active layer or an active part of it -- of the model.

DR. KONIKOW: Is that salt water at Northeast Creek?

MR. FAYE: Yes. Yes. It's not seawater, Lenny, but it's tidal. And it's definitely -- it's definitely -- it's definitely saline. Okay? Whatever that boundary is in terms of TDS or whatever you want to call salt water, I don't think it -- I don't think it quite meets that. But it's definitely saline.

DR. LABOLLE: I had noticed that the previous map you'd put up with hydraulic-head measurements, the hydraulic heads along Northeast Creek that have been measured -- or on boundaries of it --

MR. FAYE: Mm-hmm.

DR. LABOLLE: -- range from 14 to about 4 feet. And now you're putting the boundary condition on the creek of a zero head in Layer 1. How -- what kind of correspondence does that have to the elevation mapping along the Northeast Creek as far as the actual heads in -- on the creek itself, and how is that influencing the flow model?

MR. FAYE: Okay. Let me try to understand your question, which I don't completely. Are you asking: Do we actually have measurements within the various aquifers

within the Northeast Creek area or on shore at wells that were --

DR. LABOLLE: Either.

MR. FAYE: We don't have any measurements in that -- within the creek area itself.

DR. LABOLLE: I'm referring to a map you showed in the previous presentation where we were looking at hydraulic heads that shows them from --

MR. FAYE: Yeah. The estimated potentiometric surface?

DR. LABOLLE: Exactly.

MR. FAYE: Yeah. Okay.

DR. LABOLLE: And I'm looking at a contour map here in one of the reports that shows a predevelopment simulation, and now I'm hearing you describe this boundary condition of a zero head along the creek. And I'm asking how does that boundary condition influence the model because there appears to be some potential inconsistency there between the 14- to 4-foot head difference along Northeast Creek in the measured potentiometric heads. And I say along Northeast Creek --

MR. FAYE: Mm-hmm.

DR. LABOLLE: -- I mean, they're interpolated from measured heads --

MR. FAYE: Mm-hmm.

DR. LABOLLE: -- taken at wells, you know --

MR. FAYE: Mm-hmm; right.

DR. LABOLLE: -- in the land nearby and the heads plotted, for example, in the potentiometric contours in one of these predevelopment simulations. And this refers directly to the boundary that you just discussed, the --

MR. FAYE: Right; right.

DR. LABOLLE: -- zero-head boundary.

MR. FAYE: Right. The -- I think the map you're referring to, the actual loop contour is 4-feet upstream of -- that shows flow toward Northeast Creek. The actual loop contour is a 4-foot contour, not a 14-foot contour. And then there's -- you're going to have to remember now, this is an interpolation, so --

DR. LABOLLE: Well, I think it was four on the downstream and then --

MR. FAYE: That's right.

DR. LABOLLE: -- 14 feet if you go up the creek, I think, if you go to the far end of the creek. Is that -- am I correct, or...

MR. FAYE: Well, that -- yeah. That's an interpolation from a point onshore at Tarawa Terrace to a further point, further offshore -- onshore at Holcomb Boulevard. So --

DR. LABOLLE: Okay.

MR. FAYE: -- this is just an estimated -- remember, I said this was a map that we were --

DR. LABOLLE: There we go.

MR. FAYE: -- we would try to put in the highest water level so that we could just kind of define for our own purposes what we thought the major flow directions were in the system as well as what the major boundaries were.

DR. LABOLLE: I can see these Xs on here are -- or the plus signs are the actual data points used in creating --

MR. FAYE: Yes.

DR. LABOLLE: -- this map.

MR. FAYE: Yes.

DR. LABOLLE: So effectively, what I'm hearing is that you don't -- actually, you don't have enough data near the creek to --

MR. FAYE: No.

DR. LABOLLE: -- just to --

MR. FAYE: No. No.

DR. LABOLLE: Okay.

MR. FAYE: This was -- this was a kriging exercise.

DR. LABOLLE: Which explains the inconsistently.

MR. FAYE: Yeah. Yeah.

DR. LABOLLE: Okay. Thank you.

MR. FAYE: We were just there trying to -- well, for example, this shows up very nicely here; this Loop 4, I mean. It definitely shows that you're looking at -- as far as the extant data are concerned and as far as this particular interpolation is concerned, you definitely have, you know, a gaining stream. And you have -- definitely have a flow toward it from the north to the south and the south to the north.

And there's, you know, an inconsistent -- this is -- this shows the inconsistency between -- you know, caused by interpolation very well. You've got, you know, this data point here. Obviously, this contour in the real world doesn't cross the river like that. But this is all of our dirty laundry, you know, that we're laying out there, I mean. And this is just for estimating and interpretive purposes. This is nothing that we would put forth as a real potentiometric surface map.

Okay, Claudia, let's go to the modeling.

DR. JOHNSON: Bob, take about five minutes, and then we will adjourn for lunch and come back and continue with what you are presenting.

MR. FAYE: Okay. I'll try to finish as much of it as I can in that five minutes, Dr. Johnson. Thank you.

That's a picture of our grid. That's the active model domain. This is the now infamous northern boundary

that we talked about earlier. This is Layer 1 -- yeah, Layer 1. This is Frenchman's Creek. And that's an old -- this is an old map, by the way. This was before I filled in the rest of Northeast Creek as a -- as a specified head boundary.

There's your -- I forgot I had the map with me here. There you go -- layer tops or cell-by-cell arrays that equate directly to the corresponding geohydrologic unit arrays. And I just showed some examples that we've already seen. We're not going to repeat that.

I did play around with the horizontal hydraulic-conductivity distributions a little bit and try to differentiate a hydraulic-conductivity array for the Tarawa Terrace aquifer and then possibly -- and the River Bend unit and then possibly a different array for the middle Castle Hayne aquifer. But you can -- you can take your pick. It's, basically, I think, if you used all the data and assigned it to all the layers as far as the aquifers were concerned, you probably would not be far off.

Let's see. The horizontal hydraulic conductivity of Layer 9, I reduced strictly to 5 feet per day. And that was just based on a qualitative evaluation of the few descriptions of lithology of that unit that I had. I assigned a hydraulic conductivity of .2 feet per day to

all of the confining units, and that was somewhat arbitrary but not completely.

I had a -- I had a -- one aquifer test, a good aquifer test actually using an observation well. Where the observation well was -- actually both the observation and pumping were partly screened across the Tarawa Terrace confining unit. And it came out to be a very low horizontal hydraulic conductivity, and so -- I think of like 2 feet per day. So I just took an order of magnitude less than that and assigned it.

And I want to make a comment, too, about the model that I hope you'll keep in mind through the rest of the discussion. This is just -- this is a preliminary calibration that we got to where we thought we were actually getting some reasonable results.

We haven't really been able to completely test the flow model or for sensitivity or the advection transport model for all the results that were -- that we'd really be interested in. You could look at it on the other side. There's not a lot of sense spending time on that if we have a fatally failed model, so that will -- hopefully, we'll find things like that out from your panel comments.

And I think the vertical anisotropy of -- was 10 percent that I assigned to all layers. The specific yield of the Tarawa Terrace aquifer, I assigned as .2. The rest

were -- was point -- well, the rest doesn't -- don't count. That's the only unconfined aquifer.

The storativity of the model, Layers 2 to 9, I assigned as five times ten to the minus four. I have no storage coefficient data for any of the aquifers, okay, with the possible exception of one or two measurements that I kind of wonder about in the Tarawa Terrace aquifer.

But as far as the -- as far as the other layers are concerned, two to nine, the storativity is constant at .0005. The specific storage of all the model layers is simply the thickness determined from the layer geometry divided into that number, and that's our specific storage that we assigned to the model in a cell-by-cell array.

Okay. The calibration strategy. Dr. Johnson, you ready?

DR. JOHNSON: Let's stop right here.

MR. FAYE: Okay.

DR. JOHNSON: And we will resume with your presentation because it's really important that we understand what it is that's been done and what you're proposing to do. Also, Mr. Maslia has prepared some responses to your premeeting comments. And following Bob's presentation, Morris, I'd like for you to put that in front of us.

Following that, we will then begin discussing -- and

it may be simply something that reflects my own personality. But they gave us eight questions to answer, and I propose to drag us through one by one because they took the time to prepare them. And they really need your advice and insight on many of those questions, it seems to me.

So that's kind of how I see -- how we proceed after lunch. Does anyone want to do it differently, or...

(No audible response)

DR. JOHNSON: Okay. Well, be back here promptly at one o'clock because that's when we will resume. And, Morris, any questions, any announcement about the lunch arrangements?

MR. MASLIA: Again, if you want to eat at the Century Center motel or hotel where you're staying -- I've eaten there before. It's fine. I'm still around. The bus is there. I would ask that the panel members get the first bus out there because the bus seats 12. We're going to make two trips and then anyone else. Or there are other establishments around here. But we've allotted 11:45 to one -- an hour and 15 minutes or so.

Obviously, I know Dr. Johnson would prefer to get out by five today. Today's not as critical as I'm sure people who are catching a plane tomorrow afternoon, so we'll just play it by ear then. But do try to get back as promptly

as we can.

(Whereupon, a recess of approximately 73 minutes was taken.)

MR. FAYE: All right. Let's continue with the discussion where we left it off. Let's talk about the model-calibration strategy, if we could, for just a minute. The first -- the first effort was to develop a conceptual model of groundwater flow. Then we wanted to define a predevelopment condition as well as we could, knowing that it was, at best, an estimate of predevelopment conditions -- and when I say "predevelopment," that's prepumping -- and simulate that as well as we could, but knowing that we would have to iterate back and forth between a transient simulation and a predevelopment simulation in terms of changing arrays and whatever; but any -- to see if the simulations that we -- that we obtained for the prepumping condition would generally support the conceptual model and then attempt to do the same thing basically with transient simulations.

And we would have to choose the period of interest for the transient simulations as a period when we had as many water-level data as we possibly could to give us some insight into how good or how poor our transient simulations were or are. And essentially, that's -- with a few sort of rather cursory advective transport

simulations, that's -- that is where we are now in the modeling effort, groundwater-flow modeling effort.

The conceptual model that we came up with -- and I've already alluded to all of -- to most of this. Your groundwater flow occurs as -- groundwater recharge occurs in the highland areas and flows down gradient toward Northeast Creek and Frenchman's Creek and New River. The long-term average annual recharge is 12 inches, and that is -- that's borrowed strictly from several North Carolina State and USGS reports. That seems to be the favorite number that folks -- that folks apply to this part of the North Carolina coastal plain in terms -- could you go back, Claudia -- in terms of recharge to the water table.

The Tarawa Terrace area is not dissected to a large degree with drainage, with streams. Frenchman Creek is essentially the only prominent creek in the area. And my particular feeling is that recharge could probably range from 12 -- net recharge could probably range from 12 to 16 inches per year in that area. If you look at the maps of long-term average annual rainfall and potential evapotranspiration for this part of Onslow County in North Carolina, you're looking at a difference between the two numbers of about 16 inches.

So somewhere between 12 and 16 inches per year is the number that we'll probably end up with as an estimate of

long-term average annual recharge, and that's one of the things that we want to continue to -- one of the issues that we need to continue to address in the modeling that we haven't done yet.

And the other third element of the conceptual model is -- and I've already suggested that previously -- that the potentiometric surfaces in all of the aquifers are relatively similar. And if you'll recall, that large area map that I showed earlier that we had some discussion about here, if we just take the piece out of that that reflects Tarawa Terrace, you can see the data points. You can see the contours, and now these represent -- these are data points that represent the highest water levels at a particular point or the oldest. And for the most part, they're the highest.

Okay. All of these points here in the western part of the study area, these relate to us; fairly coarse and crude studies of underground-storage tank removals. And we selected these water levels regardless of season, regardless of -- regardless of season. There's probably some fairly inherent inaccuracies in there because of the lack of data that we had at a particular point. But to be honest with you, I was just so happy to have a data point in a particular place, I just -- I selected it and just kept in mind the caveats regarding the accuracy of the

point.

But that's the map in detail for Tarawa Terrace that we generated, our estimate of the prepumping potentiometric surface. And if you recall, I mentioned earlier in the context of the framework discussion, that the monitor wells and bore hole logs that we had were concentrated in the southern part of the Tarawa Terrace area. That's actually in a shopping center area there where there's a -- probably a half a dozen or so RI/FS operations going on. And then here, of course, are the monitor-well data and -- related to the ABC problem.

So that's our conceptual model, the hydraulic characteristic data that we described earlier, and the arrays and whatever. We applied that to Modflow, Modflow 2000. We have the drain -- is that the upper Castle Hayne? That is -- that's either -- well, that could be the River Bend unit or the lower unit. It's probably the River Bend unit. There's our simulation. You'll recall now that -- darn it. Claudia, can we go back, please; forward one. There we go.

Recall that in the uppermost layer that Northeast Creek out to the midchannel section is all a specified head of zero elevation. You can see that, for the most part, at 12 inches a year recharge, with Frenchman's Creek in there as a drain -- and this is -- this is three or

four layers below the Layer 1. You can see that the discharge to Frenchman Creek is still occurring. It's well defined. You can see that the -- that the head declines from the highland areas toward Northeast Creek and toward New River, toward Frenchman's Creek.

The flow lines are just as we had hoped in the conceptual model down toward the southeast and the south toward Northeast Creek. So for all intents and purposes, given the sort of cursory data and approach that we used, the simulation of the prepumping conditions, I think, supported our conceptual model quite well, and we were satisfied with that.

So let's take another look. No. That's the simulated potentiometric surface in the lower Castle Hayne aquifer. So we've essentially gone from Layer 1 to Layer 9. And as you can see, just as the conceptual model indicated, we're dealing with a very similar -- very similar directions in terms of flow lines and a relatively similar potentiometric contours and slightly higher heads; slightly lower heads in the highland areas; slightly higher heads in the discharge areas.

This is a scatter diagram of those data points that I just told you about, wherein -- which we used to construct our prepumping surface. This is just a direct one-to-one comparison between the simulated head and the observed

head with -- and the observed heads, as I said, they have some bit of baggage associated with them. But it's not -- I think that's quite good actually. The variance on this, I think, was slightly less than one; the comparison between the observed and the simulated heads, .96.

There we are. Okay. There's our simulated predevelopment budget, the recharge -- Claudia, please. Thank you. She's getting used to me.

The recharge was 1.9 CFS, and if you want to distribute that to the 1400 acres for a year, you'll find that you've got 12 inches a year. Discharge to Frenchman -- we want to distribute that then as discharge. Discharge to Frenchman's Creek was .6 CFS, and discharge to Northeast Creek was 1.3 CFS. And this is nice and easy in the model. It tells you what you're discharging to drains, and it tells you what you're discharging to specified heads. So it's sort of a no-brainer after the computation is done.

All right. We'll talk about the transient simulation. I went into some discussion in the report regarding the quality of head data that we were dealing with, with respect to creating a transient simulation, developing a transient simulation. The vast, vast, vast majority of those head data occur between 1978 and 1985. And as best as I can understand it -- and I would be the

first to admit I don't completely understand where the head data come from or how they were measured, I guess, is a better way to say it -- these are air line measurements.

And there was apparently a monthly requirement at Camp Lejeune to obtain what they called a static level and a pumping level at each of their supply wells. And we have data, as I said, from Tarawa -- for Tarawa Terrace for almost all of the supply wells. There's data gaps, but all of the supply wells are in the mix from January of 1978 to about April of 1986.

And -- so we used the static-water levels as a calibration standard, and we didn't try to adjust them. We just took them as they were. And you'll see in a slide here that, basically, these levels -- you know, for static levels, they're sort of all over the landscape. We don't have any notion of the accuracy of the gauges that they used. I made some -- I made some estimates of that in the report. We don't have any notion of the accuracy of the gauges that were used to obtain these measurements.

We do know that the gauges were calibrated to the depth of the air line in the well. We don't know if there was a standard. For example, when you obtain a water-level measurement, you repeat the measurement until you get a consistent result within some predetermined error. We don't know if that was done. We don't know whether

this measurement was just a one-time shot. We don't know how much time elapsed between turning. If it was indeed a static measurement, we don't know how much time elapsed between terminating the pumping at the well and collecting the so-called static level. We don't know any of this.

We're on track to answer some of those questions when we have some discussions with the folks at Camp Lejeune. But I just want to outline the uncertainties related to these data. So -- and we selected -- because Morris and Mr. Bove are -- you've already heard this morning of the time reference that they're interested in, we selected one-month periods as stress periods.

So between -- and we extended the transient simulation through 1994 because, in '91, '92, '93, and '94, we had several dozen accurate water-level measurements that were obtained throughout the Tarawa Terrace area in various monitoring wells that were related to several RI/FS investigations, ongoing investigations. So rather than stop the transient analysis at, like, when the wells shut down in 1987, we extended the analysis without pumping at Tarawa Terrace up through the end of 1994 to take advantage of those additional measurements.

Let's go through a number of details. So that results in 204 monthly stress periods. Because I think the 12-inch standard -- the recharge of 12 inches per year

is somewhat on the low side -- I had some difficulties with cells drying up in the upper two layers of the model, and this caused some convergence problems during the transient simulations.

So I just tweaked the recharge for that particular stress period; just would start it at 12, and I'd increase it to 13 inches a year, maybe 14 inches a year to maintain a continuance convergence for each stress period. And I had, ultimately, a range of recharge rates between 12 and 16 inches per year that I ended up using for a month. Those were monthly rates. I think the average recharge that I ended up with between -- for the period January '78 to March -- or December of '86 was like 12.7 inches per year.

We had data from a consultant's report that listed the well capacities, the active supply wells, in 1979. And those are the capacities that we identified and used throughout the transient analysis. We also had annual -- annual average daily pumpage rates. Actually, these were -- these were treated-water rates from the Tarawa Terrace WTP on an annual basis, so -- that were reported by the USGS in one of their reports.

So, for example, in 1982, for example, we would -- we had a number of, like -- I don't know. I'll shoot at it -- maybe, like .93 MGD. So for the whole year, 1982,

the average pumping rate was like .92, .93 MGD. So we had that number, and we had well capacities.

We also had a crude idea of how Tarawa Terrace operates their well systems. It's called a rotating system. They would -- at a particular well, they might pump for eight hours a day, and the well then would be on standby for like 16 hours a day. And they would rotate that type of a schedule through their whole active supply well network. And, of course, we don't have -- we have no data indicating the period of pumping for any particular well for any particular day.

So -- but I did know -- I did -- unless these operational records that had -- that we have copies of that include these static water-level measurements. Unless they indicated that, say, for example, Well TT-26 pumped all month or Well TT-52 was down for two months for maintenance or something like that, I made sure that the actual rate that I used for simulation in the model was less than the capacity and also that all of the wells pumped for a particular stress period for a particular year equaled the rate -- the average daily rate reported by the USGS. Those were the only two constraints that I had.

And a secondary constraint were the operational records. So if a -- if the records told me that a

particular well did not pump for a certain three months in 1984, I honored that. I took that pump off-line. I didn't -- that well off-line. There was no water discharge for that.

So those are basically the three constraints that I used to put together a pumping schedule for 1978 through 1986. And then, of course, when the wells were all shut down in March of '87, then all the wells were turned off. And the Tarawa Terrace -- then the aquifer basically recovered to pretty much its simulated predevelopment condition in a very short period of time.

Okay. I think that covers that all.

DR. WALSKI: I have a question.

MR. FAYE: Sure.

DR. WALSKI: On the monthly recharge rates, did you take into account anything about whether it was a wet month? dry month? Like, some --

MR. FAYE: No.

DR. WALSKI: -- months you had hurricanes hitting with --

MR. FAYE: No.

DR. WALSKI: -- huge flows --

MR. FAYE: No.

DR. WALSKI: -- and some with none.

MR. FAYE: That's a great question, Tom. No. We

haven't had time to do that. We're in the process of having discussions, actually. And that's something that we would very much like to hear from you -- from you-all, from the panel. What we have in terms of meteorological data: We have pan evaporation data so -- and on a monthly basis. We have rainfall data on a monthly basis for our whole period of interest, basically from 1950 to 1995, something like that, as much as we want. Okay?

So we have that all on a monthly basis. And once we can make a decision about a long-term average rainfall -- rather long-term average recharge, whether it's 14 inches or 13-1/2 or 15 or whatever it is, we're trying to devise a scheme to use this meteorologic record to adjust our recharge on a monthly basis. That's clearly, clearly on the radar screen, but as I said earlier, these simulations were pretty basic. I mean, we're just trying to get a handle on things, and we haven't done that. Okay?

And that's kind of why I felt free to just kind of tweak recharge during a stress period when I had a convergence problem, just boost it a little bit to a particular higher rate -- a little higher rate and achieve convergence and go on because I wanted to see what the end product was. Okay?

DR. KONIKOW: Did you give any thought to the possibility that recharge may be greater than the natural

recharge in urban areas where you have lawn watering and --

MR. FAYE: Yeah.

DR. KONIKOW: -- leaks and --

MR. FAYE: Leaky pipes --

DR. KONIKOW: -- car washing and --

MR. FAYE: Yeah, we have; we have. And any comments that you-all have about how to deal with that -- there's a really good paper -- I can't quote it right now to you -- that really goes into a tremendous amount of detail on this and using GIS to look at the lawn areas and the paved areas and everything else and --

DR. KONIKOW: Are they on septic tanks, all the houses --

MR. FAYE: They were.

DR. KONIKOW: -- housing developments?

MR. FAYE: They were originally on septic tanks.

DR. KONIKOW: That's a source of recharge.

MR. FAYE: Oh, absolutely; for quite a while. And they're on a collection system now, but for --

DR. KONIKOW: A leaky collection system, no doubt.

MR. FAYE: Probably; yeah. And the water supply, the pressurized pipes are probably leaking as well in different places. Yeah. Yeah. Yeah. We've thought about all of that. We haven't really acted on it. We're

in the process of trying to find -- figure out how to act on it.

DR. KONIKOW: Now, you have a lot of cells going dry, I saw, in your simulation --

MR. FAYE: In the -- in the -- yeah --

DR. KONIKOW: -- if you're concerned about that.

MR. FAYE: In the -- in Layer 1 and Layer 2 in the highland areas; yes. And that -- and I know for a fact that that actually is true in the real world. These -- those cells would only be wet, seasonally wet. Okay?

DR. KONIKOW: Yeah.

MR. FAYE: The water table --

DR. KONIKOW: Did you -- did you run Modflow with the rewetting?

MR. FAYE: I did, and it just caused a tremendous amount of convergence problems. I'm going to revisit that again.

DR. KONIKOW: Have you thought -- you were using monthly stress periods, but I believe you're also using monthly time steps. Have you thought of cranking down your time-step size?

MR. FAYE: Oh, to a smaller size?

DR. KONIKOW: Yeah. In other words --

MR. FAYE: Yeah.

DR. KONIKOW: -- you could have monthly stress

periods but --

MR. FAYE: I did that. I did that. I did that when I rewet it, when I played around with the rewetting feature. And it just -- I was not -- I spent a lot of time. I was not successful. I'm hoping -- I'm hoping -- well, I very strongly believe that the baseline recharge that we come up with, this long-term average annual, is going to be somewhere probably around 14 inches or so. I'm hoping that when we're dealing with that extra recharge plus, you know, we'll be starting out as a prepumping condition. So we'll have antecedent conditions taken care of pretty well, right from the get-go, in early 1950s.

I am hoping that we -- we're still going to have dry cells. I'm hoping it's not going to be a big issue. And I hope, maybe, we can try to do some rewetting in that context, but the rewetting was not at all successful, not at all.

DR. KONIKOW: Maybe, with smaller time steps, it would work better.

MR. FAYE: It could. It may. I definitely did try that, but I'll definitely try it again.

DR. KONIKOW: Yeah.

MR. FAYE: I'm open for any -- I'd like to have that rewetted. I really would.

DR. LABOLLE: My experience has been, like Lenny's suggesting, decreasing the time step --

MR. FAYE: Right.

DR. LABOLLE: -- but you can also -- if you want to get that to converge, another helpful item is to use a solver with a dual-convergence criteria. So in other words, you'll have convergence criteria for the outer, nonlinear loop, in which things are --

MR. FAYE: That's the PCG solver.

DR. LABOLLE: -- which you can -- which you can -- no; not the PCG. It will be the -- actually, it will be one of the latest solvers that Mary Hill released. I forgot which one it was. It's the only one with the dual-convergence criteria.

MR. FAYE: Okay.

DR. LABOLLE: I can send you one for the PCG if you want. I have one.

MR. FAYE: Oh, that'd be great.

DR. LABOLLE: But the nonlinear loop, you set its loose convergence criteria, and you can set the linear solver. You know, it's a very strict convergence criteria, and the combination of the two allows you to --

MR. FAYE: To rewet?

DR. LABOLLE: No. What it allows you to do is to solve a confined flow problem as an approximation

essentially is what you end up doing because really you're solving a confined flow problem --

MR. FAYE: Mm-hmm.

DR. LABOLLE: -- at some point in time. And you're looping nonlinearly, but you --

MR. FAYE: Mm-hmm.

DR. LABOLLE: -- at every point, you're making a confined approximation, essentially. Anyway, that allows you to converge. That's one issue. And another comment I have is on your calibration, recognizing that it's preliminary, but I noticed that if I were to probably fit a line through the scatter points there that it would probably have showed less of a gradient. And I think that --

MR. FAYE: You mean the scatter line?

DR. LABOLLE: Yeah; exactly --

MR. FAYE: Yeah.

DR. LABOLLE: -- and then the one-to-one.

MR. FAYE: It would --

DR. LABOLLE: And so the implication being that your heads up here --

MR. FAYE: Mm-hmm -- are too low?

DR. LABOLLE: -- out in front are lower than --

MR. FAYE: Yeah.

DR. LABOLLE: -- you expect, and --

MR. FAYE: Yeah; yeah.
DR. LABOLLE: -- bringing them up --
MR. FAYE: Yeah.
DR. LABOLLE: -- relates to this --
MR. FAYE: And that's the --
DR. LABOLLE: -- wetting and drying --
MR. FAYE: And that's the recharge problem too.
DR. LABOLLE: Exactly.
MR. FAYE: That -- I know that, and I'm hoping, again, like I say, that the baseline recharge, whatever we actually end up with is going to be more than 12. And it'll take -- and you'll see on the -- you'll see on the scatter diagram for the transient analysis the same kind of thing, I believe, although it's only the latter part of it up toward the top where we have some really decent data that it shows up. But I'll point that out.

Here's the capacity data that we used. This is from the consultant's report, that I mentioned, in 1979. And I violated this with respect to one well. After like 1980 or something like that, I violated that with respect to TT-53 or 52, I believe -- it's in the report -- just because I couldn't find any water anywhere else. I needed water to match the USGS criteria.

It was one of those several periods -- several month periods where several well -- two wells were down. And I

just needed that extra water to match that annual rate, and so I violated that criteria at that time for Well TT-53, I believe it was, or 52. But that was the only time.

All the other times, those capacities were honored to the limit. In other words, unless I had a note that the well was being pumped for 24 hours, all of the capacities that I used in the model to pump were less than those recorded there and in many cases substantially less.

DR. POMMERENK: Bob?

MR. FAYE: Yes.

DR. POMMERENK: The map shows a lot more wells than you indicate here.

MR. FAYE: Yes.

DR. POMMERENK: Do you have the data for the other wells as well?

MR. FAYE: A lot of them we do, Peter.

Can we go back to that one, Claudia. Is it in -- is it in this module where I showed the -- yes. Keep going. There it is.

Yeah. Yes. Yes, Peter. These TT-45, TT-29, TT-28, 2-A, TT-55, TT-27 were all out of the -- out of operation by 1978. Okay? These are some of the original wells along with TT-26 that originally supplied the Tarawa Terrace network water supply treatment plant: TT-27,

TT-55, 2-A, 28, 29, and 45. And in the very beginning of Tarawa Terrace, from about 1952 to 1961, there were actually two wells, and Tarawa Terrace call -- or Camp Lejeune called them six and seven that were off the reservation. They were off-campus. They were about a mile and a half or so up Bell Forks [sic] Road.

And what the operation was there, I have no idea how the water was actually connected to the network at Tarawa Terrace. I don't know. But they're officially listed as Tarawa Terrace supply wells in the records, numbers six and seven. And they're actually located on Bell Forks Road, and I have a crude map showing where they were located.

So there's another actual two wells that actually don't show up here for the very early supplies. Now, you have to remember those -- all of these wells were off -- out of the system by about 1961 -- those ones. Except for TT-26, all of those wells were out of the system by 1961 or '62. Okay?

DR. KONIKOW: Why were they out of the system?

MR. FAYE: Pardon?

DR. KONIKOW: Why were they taken out?

MR. FAYE: The early wells, Lenny, the way they were constructed had a tendency to sand up. The maintenance was a horrible situation. They had that, plus, I believe,

there were some network problems because of the lack of proximity to the wells, to the WTP. The WTP is located about right there.

And so they just -- they took those wells out of the system. They were low producers. I have records in 1959, indicating that they were very low producers and -- except for TT-26. And so in '61, they came in and put in a number of additional supply wells and took those all off-line, abandoned them.

Thank you, Claudia.

DR. POMMERENK: I have another question on that table that you showed earlier.

MR. FAYE: The Von Oesen table?

DR. POMMERENK: No; the capacity table.

MR. FAYE: Yeah. Could you go back.

DR. POMMERENK: According to those numbers, they would have to meet their one MGD daily demand to operate --

MR. FAYE: Easily; easily.

DR. POMMERENK: -- three wells for 24 hours?

MR. FAYE: Mm-hmm; easy.

DR. POMMERENK: Or let's say six wells for 12 hours --

MR. FAYE: Yeah.

DR. POMMERENK: -- because the state of North

Carolina doesn't allow you to run your --

MR. FAYE: Right.

DR. POMMERENK: -- wells 24 hours a day.

MR. FAYE: Right. Well --

DR. POMMERENK: So how did you determine in your model which out of those seven wells -- did you just have them all run at a, you know, prorated capacity?

MR. FAYE: No. What we had, Peter -- we actually had copies of tables from Camp Lejeune of their operational records. Okay? And the various columns of these records would show a pumping level, a static level, a pumping rate, operational notes about the well, whether the well was down, whether the well was -- where the pump was being replaced, things like that. And we have those on a monthly basis from January '78 through March of 19 -- or April of 1986.

So the pumping schedule that is used in the model for each of the 204 stress periods honors those operational records 100 percent in terms of what wells were operating, what wells were not. I could see that what I just said is bothering you. What is that?

DR. POMMERENK: No. I'm just wondering. So that's in the simulation. And I'm not a groundwater modeling person, but the simulations of those wells that you determined according to that operating schedules were

operated --

MR. FAYE: Mm-hmm; at that month.

DR. POMMERENK: -- for the whole month.

MR. FAYE: Yeah. I had to. Yeah.

DR. POMMERENK: Okay.

MR. FAYE: That's our -- that's our minimum --

DR. POMMERENK: And at that capacity?

MR. FAYE: No, no, no, no; because, I just said, the wells rotated. They were, like, on-line eight hours a day and off like 16. So if you -- if you use that capacity --

DR. POMMERENK: You were just --

MR. FAYE: -- you're assuming a 24-hour pumping period.

DR. POMMERENK: No. It's understood. Thank you.

MR. FAYE: Okay. Okay. So that's what I said. The pumping schedules in the model honor those capacities, such that the rate was always less --

DR. POMMERENK: Okay.

MR. FAYE: -- than that capacity.

DR. POMMERENK: It's understood. Thank you.

MR. FAYE: Okay. And I mentioned that the USGS gave us average daily rates for various years. And the -- our -- the pumping schedule, Peter, also honors those rates from 1978 to '86. And then '87, you know, everything went to hell, and they shut it down.

And I mentioned the static water-level data. This is -- this is Well TT-26. This is what these data look like. These are the static measurements, unvarnished. That's what they are, and that's typical of all of the so-called static measurements for all of the supply wells.

Okay. Given the schedules, given the data that I've talked about, that's the scatter diagram for the transient analysis. And these data here -- oh, why do I do that? Thank you, Claudia.

These data here are -- for the most part, a lot of these or the majority of these are the monitor-well data that we had for the early nineties in various parts of the -- of Tarawa Terrace. Almost -- and these are all of these so-called static water levels that we just discussed.

These are the accurate measurements here. And we have a situation where, for example -- and I don't understand this at all. Like, for example, like, at TT-30, which is near TT-26 and TT-25, all of a sudden in, like, about 1980, the static water levels just go up and stay there. And the well is running. The well is operating, and I don't know what happens. Then it just -- water levels rise, and it stays there. Not only is that pump -- is that well operating, but it's near two other operating wells. And yet -- but those numbers are in

there. We didn't -- I didn't selectively disregard any of the data at all. It's all there.

DR. JOHNSON: Bob, you need to kind of wrap this up, please.

MR. FAYE: Okay. We're almost done. And I'll just show you a couple of the results. This is TT-26. That's the observed -- so-called observed static and the simulated. There's TT-31, -52, -67. And there's the stress period '84, when TT-23 was operating and just very rapidly that -- and we've just done some very preliminary advective transport simulations. And let me go through that.

There's our water budget for the stress period '84. There's our recharge. It was 12.8 inches a year, what went into storage. That's induced recharge from Northeast Creek, which would have been brackish water; our well pumpage, and that honors the USGS rate for 1984; the discharge of Northeast Creek; discharge of Frenchman's Creek; and change in storage.

Advective transport, I just basically did several things. We -- I seeded the cells or one or two cells right next to ABC One-Hour Cleaners to see where they would end up. Because of the -- because of the contaminant extent that went north and west of ABC Cleaners that we saw on the maps before lunch, I put

particles in 600 feet west of ABC Cleaners along Lejeune Boulevard. That's State Route 24, and I looked at the time of travel to the Tarawa Terrace supply wells of interest. And I came up with an explanation for the occurrence of PCE at Well TT-23, which is that isolated section to the south that we looked at in the maps earlier.

When we seeded the particles right in the immediate vicinity of Tarawa Terrace -- of the ABC One-Hour Cleaners, all of them were captured at TT-26; everything. The -- none of the other supply wells captured anything for this particular stress period '84, which relates to December of 1984. When we went a little bit west of ABC One-Hour Cleaners -- and this is after 10,000 days, by the way -- indeed, TT-23 captured particles that were seeded west of the ABC Cleaners.

DR. LABOLLE: Bob, are you running the hydraulic static then? Because you keep mentioning the stress period in '84, but then you run it for 10,000 days.

MR. FAYE: Yeah.

DR. LABOLLE: Can you elaborate? So steady-state hydraulics, transient?

MR. FAYE: The gradients, velocities, and whatever relate to that one stress period, stress period '84.

DR. LABOLLE: That would explain probably the sole

capture of contaminants in a single well.

MR. FAYE: Well, actually --

DR. LABOLLE: If you consider all the pumpage, you tend to have things --

MR. FAYE: Yeah. It could bounce around. Yes, it could; in reality, yeah. I also did it for other stress periods, but I came up with slightly different configurations in terms of drawdown from the, you know, in the system. And TT-26 captured everything, always captured everything when -- but, again, that's a simulated of continuous pumping. But it captured everything that I put in right in the immediate vicinity of ABC Cleaners. It captured everything. It always went there.

DR. DOUGHERTY: Were these all seeded in the top layer?

MR. FAYE: Some of them were. One experiment seeded Layer 3, which is the River Bend unit. And that's where a lot of the contaminant was -- has been observed. And I also seeded Layer 5, which is the lower unit of the upper Castle Hayne aquifer. And there was a little bit of contamination observed in that layer as well from the field data. So I seeded both layers.

DR. KONIKOW: Why didn't you seed layer -- the top of Layer 1? That's where the contaminants reached the water table.

MR. FAYE: Yeah. The -- that's a good question. The -- at that time, the Tarawa Terrace, when the data were collected, all of the -- all of the contaminant was below that particular layer. And that was -- that was when I was having problems with the cells drying out too, Lenny, in Layer 1. And that's up in the highland areas with Layer 1 and Layer 2. So I ended up -- I ended up seeding Layer 3.

DR. WALSKI: The fraction of the time was 26 on? Is it run like 80 percent of the time, or did it run 70 percent of time on average?

MR. FAYE: That, I really don't know, Tom. All I know that it probably rotated --

DR. WALSKI: Okay. So --

MR. FAYE: And so didn't run 100 percent of the time.

DR. WALSKI: So therefore, you can explain possibly some of this water getting past it by the fact that, if you took real, like, hourly time steps for a change, the hydraulics would then shoot past it and --

MR. FAYE: And that's right; that's right. That's right. And there's even a better explanation, I think. Okay? And that's this right here. If you seed -- there's another well down here, TT-54, right here. And TT-23 is actually right here, and if you look at the capture zones of TT-26 and TT-54, you can see right in this area that

they're very close to one another. So when the well -- when TT-26 is shut down for any reasonable period of time, probably the capture zone for TT-54 moves over into part of the capture zone for TT-26.

Also, this is a highly contaminated area right in here. This is a much less contaminated area here. So even if this situation here persisted through time constantly, I think you may also have had some exchange of mass along concentration gradients from the highly contaminated area to a lesser contaminated area. And it would end up in the capture zone of TT-54.

Now, you say, how did well -- well, this -- you have to understand that TT-23, at best, only operated for about a year. And TT-23 is right here. And in the DPT analyses that we have, there was a low-level PCE contamination throughout all of this area here.

So my conclusion was that one possible explanation for the occurrence of PCE at TT-23 was not that TT-23 pumped for six months and was able to capture PCE that was in the general vicinity of ABC Cleaners, but rather over a period of time -- TT-54 began operation in 1961. But rather over a period of time, you had intermittent capture of PCE by TT-54 that ended up creating this low-level contamination in this particular area of the Tarawa Terrace campus or housing area.

And then in 1984 when TT-23 was actually turned on for a short period of time, there was a resident PCE in the aquifer that was induced into the well. That's one -- that's my explanation, and I'm sure there's others. But that's my explanation for the occurrence of PCE in Well TT-23.

DR. DOUGHERTY: Quick question. In terms of -- I want to connect this one to the pumping capacity chart from Van Oesen. Looking at those capacities for the late seventies, it appeared that if I summed up the capacities for the TT-26 area, there are the three wells up there --

MR. FAYE: Mm-hmm.

DR. DOUGHERTY: -- and then for the cluster that's down in the development that there was a significantly larger net capacity for the southern cluster than the northern cluster --

MR. FAYE: No.

DR. DOUGHERTY: -- is that accurate? I mean, it was a partial record.

MR. FAYE: It's as accurate as I know it.

DR. DOUGHERTY: No. What I'm saying is my assessment, since I only saw this table rather than the entire simulation set of data. In terms of what you simulated, did you actually have twice as much pumping from the southern cluster of wells than from the northern

cluster? Is that roughly the division?

MR. FAYE: Oh, I see; because of the -- because of variations that I made in the pumping schedule to honor those two criteria that we talked about; yeah.

DR. DOUGHERTY: Because of capacity --

MR. FAYE: Yeah. Mm-hmm. And -- but, again, now, Dave, you have to understand that there would be months when these -- some wells were out of --

DR. DOUGHERTY: Sure.

MR. FAYE: -- operation. So I had to increase the pumpage at other wells to make sure I could maintain that rate.

DR. DOUGHERTY: No. I understand. I've got that right. I got how it worked.

MR. FAYE: Great; okay.

DR. DOUGHERTY: But I'm just trying to get a sense for -- a simplified sense because there's an awful lot of material here.

MR. FAYE: Okay.

DR. DOUGHERTY: Basically, you're pumping twice as much down here, generally speaking --

MR. FAYE: Right.

DR. DOUGHERTY: -- than up there?

MR. FAYE: Right. But if you -- and I -- what I also looked at the simulated capture zones for all of those

wells. And they're all deflected up to the northwest except for TT-54. Okay? These wells down in this, they're all deflected up here --

DR. DOUGHERTY: Mm-hmm.

MR. FAYE: -- rather than giving any competition to TT-54 or TT-26 up there.

DR. DOUGHERTY: Mm-hmm.

DR. LABOLLE: Did you look at the sensitivity of the simulated capture to vertical hydraulic conductivity at all?

MR. FAYE: No; haven't done that at all. It's on the radar screen; just there's all kinds of sensitivities that we need to deal with.

DR. LABOLLE: Yeah. It's been my experience in situations like this that it tends to be highly sensitive because what will happen is that if your source is seeded in Layer 1 and your vertical hydraulic conductivity is decreased, then the contaminant's going to migrate along more -- not in the ambient gradient, but more of an ambient gradient --

MR. FAYE: Right.

DR. LABOLLE: -- than is affected by the --

MR. FAYE: Right.

DR. LABOLLE: -- by the actual pumpage in the deeper layers, assuming these wells are screening deeper.

MR. FAYE: Right. Well, also, too, we're dealing with, in the real world, a heck of a contrast in densities. I mean, 1 to 1.6 and that -- none of this shows up in any of that simulation there. I mean, that's just strictly advective transport.

Thank you very much. And I'm sorry that -- oh. Okay.

DR. KONIKOW: When you talk about a density contrast, you're talking about --

COURT REPORTER: Please get on your mike.

DR. KONIKOW: When you're talking about a density contrast, you're talking about pure phase?

MR. FAYE: Yeah; absolutely; yeah.

DR. KONIKOW: But we're not looking at the movement of the pure phase, are we?

MR. FAYE: No. No. But, I mean, that's just -- I know it's a DNAPL. Okay? And that's what -- that's what the -- that's what it is: 1.6 in the laboratory.

DR. LABOLLE: But not at these concentrations.

MR. FAYE: No.

DR. DOUGHERTY: I wonder if the hydrodynamics will drive it.

DR. LABOLLE: Only near the source --

MR. FAYE: Right.

DR. LABOLLE: -- might we have some kind of density

effects.

MR. FAYE: And most of that is actually in the -- I mean, there is no -- the almost free product stuff is in the unsaturated zone at the source. And there's a map in your report that shows that.

DR. JOHNSON: Well, thank you very much for your presentation, and --

MR. FAYE: Well, thanks for your forbearance.

DR. JOHNSON: -- also thanks to the questions from the panel. Let's proceed. Morris, you had prepared some responses. Yes, please.

DR. CLARK: I had one question.

DR. JOHNSON: Of course.

DR. CLARK: We had a side conversation before, earlier today, about the other sources of groundwater contamination that existed in the Camp Lejeune area, and I thought it might be useful for the panel to hear about some of that.

MR. FAYE: You mean, like, in the Hadnot Point area?

DR. CLARK: Well, in the Hadnot Point area.

MR. FAYE: Am I going to steal your thunder on that, Morris?

MR. MASLIA: No; no.

MR. FAYE: Okay. Yeah. I'd be happy to as long as -- the -- as Morris mentioned this morning when we first

started the program, we deliberately chose Tarawa Terrace because, believe it or not, it's the simplest system that we had to deal. Okay? As he said, there's one source, and it's an identified source as far as the contamination of the groundwater is concerned.

If you go south to the Hadnot Point area, you're dealing with dozens and dozens of sources of contamination, some relatively small, some off the radar screen, that have contaminated groundwater in a big way. A number of these sites have RI/FS operations ongoing right now in terms of remediation. We're looking at a lot of TCE, a lot of BTEX. It's kind of a mess. Okay? You're looking at -- you're looking at surface sources. You're looking at buried sources.

You face the possibility of -- you face the possibility of a particular supply well capture zone collecting contaminants from several sources very easily. So that's an exceedingly complex condition to try to do what we're trying to do. And you sort of have to crawl before you can walk. And our thought was if we can be reasonably successful, create a technically defensive -- defensible product at -- ah, a Freudian slip -- product for Tarawa Terrace, then we may have a shot at doing something similar for the Hadnot Point area. Does that -- does that cover --

DR. CLARK: But the chances of actually being able to do that, I gather, are marginal at best; right?

MR. FAYE: I really -- I don't know one way or the other on that. I would just -- in fact, I don't even know how we would approach that, maybe just a single supply well at a time. Okay? I don't know. It's just -- we're just going to have to deal with that when the time comes.

MR. MASLIA: Let me, if I might, qualify that because when Bob and I got together, again, we made the decision based on, you know, consulting work, the USGS work, and all that, that we had the best chance from -- for developing a framework and either before you even get to the modeling at Tarawa Terrace. And so that's some of the -- I guess one of the questions we've posed is: Do we extend that? And, again, it means going back to developing the geohydrologic framework again for Hadnot Point, which we -- I don't believe we've done at this point --

MR. FAYE: No; just for Holcomb Boulevard.

MR. MASLIA: -- at this point yet. And so that's one of the issues we really want to discuss. Or is it just going to be so completely uncertain and variable that we may not be able to narrow any of the uncertainties, stuff like that? So Tarawa Terrace, we felt, was our best shot, given the time frame, given agency constraints, budgets,

and time lines for the epi study. Dr. Bove can address the study time frame and some of the pressures associated with that to try to get some answers in a reasonable amount of time.

Am I on?

DR. JOHNSON: Yes, you are.

MR. MASLIA: Okay. Okay. I'm a little shorter than Bob. It's happened all my life. I even have to look up to my son, so...

In reviewing the premeeting comments and, of course, I've had a few days to look through them and hit more of the salient points. And they do bring up some gaps, if you will, that we need to address. But I wanted to give the panel a sort of a feeling that, again, we take these very seriously. Some of them may, in fact, change our approach or change our direction.

So I wanted to try to see what general areas the comments from the panel got into and, you know, what our response -- obviously, in a generalized, given the time frame that we've put these in. So I will go through here, and I'm not sure if I've included that in your handouts or not, in your packets. If not, we can get the panel a copy of our generalized responses.

But from the groundwater side, and, Doctor, did you just want me to end on the -- for the groundwater for this

morning and then --

DR. JOHNSON: Yes.

MR. MASLIA: -- tomorrow we can or --

DR. JOHNSON: Yes.

MR. MASLIA: Okay. On the groundwater, a lot of comments resided in the area of uncertainty of geologic and aquifer parameters as we've discussed thus far and what -- it looks like some mention of probabilistic methods, such as Monte Carlo, looking at realizations. And I know Dr. LaBolle has a lot of expertise in that area and has worked on some sites for ATSDR in that area.

And that is something, I think, would be the next step. The question, I think, for the panel would be: In taking that as the next step, should that be the next step prior to doing any more refinement of the Tarawa Terrace model? Should we jump into probabilistic uncertainty methods now, rather than doing any more refinement on the Tarawa Terrace model?

Secondly, some parameter estimation methods to look at sensitivities like vertical hydraulic conductivity relative to other parameters. Again, that is a direction we definitely need to go in and anticipate going in. As far as modeling boundaries and sources, source conditions, I think the best way may be to look at use of sensitivity analysis to assess the nearness or the impact of moving

that northern boundary further away from the source and seeing how much change it provides to the model, adjusting the boundary.

Again, we have the contradiction, if you will, that you've got the DEM that I didn't get to mention. The DEM data that was contoured for us -- actually, North Carolina district office is who we sent it up to, to pull it off the DEM site and provided us with the 2-foot contours, but, again, based on that and the topo maps. But I think that would be an area of -- that we could at least try to address and looking at the sensitivity of the northern boundary with relation to what impact it may provide on the model.

And the one question is: Would we see a bigger impact or a more pronounced impact if we go to the full fate and transport as opposed to just looking at the advective flow, which we're doing right now? In other words, you may find a changed impact when you go to the full fate and transport where you're looking at dispersive properties and start moving boundaries away from the ABC Cleaners' source.

The other approach -- and I think this comes into if you want to put in the area of sensitivity analysis -- is we do have techniques. Actually, there have been some papers on that, developed out of the multienvironmental

media simulations lab at Georgia Tech, but they -- where they have taken observed concentration values and backed out source locations through use of genetic algorithms.

And that's, again, maybe an avenue to explore, taking some of the observed values that we have, historical in nature, and seeing if, in fact, it backs out the source location that we are assuming to ABC Cleaners. And I don't know -- I don't want to put Dr. Aral on the spot there. But we've had some preliminary discussions on that. And as I said, that's another area that we may -- that perhaps, we should explore is using the observed data --

(Projection screens withdrew to the ceiling.)

MR. MASLIA: I didn't -- is it time? You may have to touch the touch screen, Claudia. The touch screen may have timed out (laughter). Either that, or it didn't like the answer I gave. Okay. I don’t know. Okay. You may have to hit "dual projector" to do that. And if not, I don't know if Ann Walker or somebody out in the hallway can hear us. They may have to call somebody to come get us. But I'll proceed in talking as we go on.

So those two areas of doing -- I'm not sure -- inverse modeling is not the correct nomenclature, but reverse modeling of going from the source, observed source, backing out. And that may also give us an

indication if, in fact, that source -- where we think ABC is too close to a boundary.

The next groundwater, I've got fate and transport issues. And I know, Lenny, you brought that out that we mentioned fate and transport only provided advective, and it's been our intent all along to do a full fate and transport. And again, in the Tarawa Terrace area being PCE is the only known source that would give us a single constituent model. So we are -- definitely, that's on the plans. That's always been on the plans to do that.

One of the issues I want to bring up -- and Bob mentioned -- some of the data that we get in pieces as far as production and things like that, although we've been at this for over a year, I think, more or less. For example, last week, I just got a pile of information: month-by-month, raw water, finished water, production data from Camp Lejeune from 19 -- what was it?

MR. ASHTON: 1980.

MR. MASLIA: 1980 through 1986.

MR. ASHTON: '84.

MR. MASLIA: '84; month by month. And, of course, we've been asking for all data, so I'm saying it's slowly filtering in. It may take a more direct involvement of, you know, giving ATSDR staff or whatever to going into the vault, locating contract numbers, and things like that.

But this is new data that we just were provided with from the folks at Camp Lejeune. So again, that's in that critical period. What we really still need is the prior to the '78 information; '68 to '78. We're still looking for that.

Let's see. So again, the advective transport were viewed as preliminary estimates; get the model working; any issues with -- as far as not model code, but implementation of the code that we could take care of at this end and then taking comments, feedback, from the panel. Again, at least we've got some basic parameters and basic numbers to then go into uncertainty areas, go into other more refinements of the model.

So that's really the groundwater issues; a quick preliminary perusal from your comments that I saw, and that's the direction we're going in. And we will try to answer, you know, anything else.

DR. JOHNSON: Did anything you just heard raise concerns, or is there anything that you heard for which you would give a strong endorsement? What I've heard from Mr. Maslia is a series of considerations, and all that's good. But is it something that really that you've heard you'd say, "This really ought to be something you pursue," based upon his responses?

DR. DOUGHERTY: I think you should move the northern

boundary and skip the sensitivity.

MR. MASLIA: Okay.

DR. DOUGHERTY: Just do it. Topography does not define hydraulics, unfortunately.

MR. MASLIA: And would you then just use a generalized, head-type boundary or inflow boundary since --

DR. DOUGHERTY: I'd have to look further north than the maps that I have here show me --

MR. MASLIA: Okay.

DR. DOUGHERTY: -- so I can't answer it really.

MR. MASLIA: Okay.

DR. WALSKI: Are there municipal wells, other things up north?

MR. MASLIA: Oh, yeah. There's the city of Jacksonville is, you know, pumps the wazoo out of groundwater. And I think we uncovered some -- did we not uncover some documents when we first went to Raleigh about discussions back and forth between Camp Lejeune and the city of Jacksonville about --

MR. FAYE: For the period of time that we're interested in, the pumping at the city of Jacksonville is not an issue. They have for decades pumped from the Cretaceous aquifer system, which is well below the Castle Hayne units that we're talking about here and with no

effect on the Castle Hayne.

Just most recently, they've applied for permits within the last year or so to develop wells in the Castle Hayne. But for the period of time we're involved in, Jacksonville pumping would not be an issue.

What would be an issue would be a lot of older subdivisions and industrial areas and business areas north of there that back in the fifties and sixties and seventies, the period of time that we're interested in, would have been self-supplied. And I don't -- it would be just -- we could certainly look, but I wouldn't be too hopeful of determining or of finding out what kind of -- we would know less about those situations than we would about the Camp Lejeune pumpage.

So that's the situation there in terms of the -- and that self-supplied pumping was almost invariably from the same aquifers that we're dealing with because they were shallower and they were good. They yielded good water to wells, and, of course, the businesses and the residences and everything loved that because it was much cheaper than going deeper. So that's what we're dealing with.

MR. MASLIA: I've got half a screen -- half a room screen working, and we've got a number for the room operator. So we're trying to...

DR. JOHNSON: Based on what's on the screen, we've

had one comment from David in terms of his view and strong recommendation. Does the panel have other recommendations based on what's on the screen or what you have heard?

DR. KONIKOW: Well, I would look again closer at the vertical hydraulic conductivity, its relation to the horizontal, and also the hydraulic conductivity of the clay layers of the confining units. The values that you or Bob gave earlier just seem a little too high, relative -- you were talking about .2 feet per day, as opposed to, you know, maybe 10 or 15 in the aquifer.

That -- for a clay confining layer, that just seems too high. And one of the things that might -- what you might find is that, as you make the vertical hydraulic conductivity lower and the hydraulic conductivity of the confining layers lower, your cell drying problem may go away.

MR. FAYE: Yeah. That's a good point, and you easily could be right. But the fly in the ointment there, Lenny, two things: The, admittedly, very limited lithologic -- good lithologic descriptions that we have of these confining units, yeah. They're clay, but they are very, very sandy. They are definitely sandy. And they're not real competent clays there, texturally.

I mean, when you look at the drilling times and the drilling records, pha-phooonk, I mean, it's -- you know,

there's no -- there's no slowing down at one -- at a clay. So they're leaky. They are very definitely leaky. We haven't done any kind of sensitivity analysis at all on the anisotropy or the horizontal hydraulic conductivity. But this is not, you know, this is not a -- these are not real competent confining units at all. Okay?

MR. MASLIA: I, actually -- and this is part of our question, so I don't know if you want me to pose that now. Dr. Johnson, I'll let you go down the list. But I'll just throw it out there, and then you can decide. I'm not usurping your power as the Chair.

DR. JOHNSON: I have no power as the Chair, nor do I want any. But I am fully committed at some point today to start down this list of questions, and we will do that in the not-too-distant future. Are there any other points here of emphasis from the panel on Morris' presentation? Yes, Vijay.

DR. SINGH: I think it was pointed in prepanel meeting discussion as well as during the presentation. I think that there has to be a better accounting of recharge, especially when you are doing the transient groundwater modeling because recharge constitutes the input. And if your input is not properly accounted for, I don't think -- I don't think you will be able to do as good a job in groundwater modeling. And I think that may

also partly explain the problems that you're encountering in the convergence.

Dr. FAYE: You're exactly right. I mean, we have recognized that, and I know it sounds kind of lame. But the actual truth is that we just haven't had a chance to really address that issue in a lot of detail, but I fully agree with you. And hopefully, that will solve a lot of these problems.

DR. SINGH: And the other point that I think it will be important to also evaluate the reliability of the model results, and this is particularly useful from the standpoint of giving the information to the public.

MR. FAYE: The reliability of what, sir?

DR. SINGH: The reliability of your model result, how -- what level of credence can you really put, given all the uncertainty associated with your hydrogeologic description, your parameter estimation, you know, groundwater conceptual assumptions, and a whole host of other things. I think it's very important to --

MR. FAYE: To qualify.

DR. SINGH: -- give the level of confidence --

MR. FAYE: Right.

DR. SINGH: -- or the confidence bends to the model results so that -- so that the public can have some confidence --

MR. FAYE: Absolutely.

DR. SINGH: -- in the results that you are giving.

MR. FAYE: And that should not be a qualitative assessment. That should be a quantitative assessment as much as we can do, and I fully agree with you.

DR. JOHNSON: In that same vein, I asked a question earlier about validity of the EPA models, and to my knowledge, they're quite good. So I'm not -- I don't have any agenda here other than the fact to say to you that the National Academy of Sciences has begun a very serious study of the EPA system of modeling and validity of specific models. Now, I do not know how far into that study they have gotten, but I surely do know that they are doing that at the request of EPA, which is quite commendable.

MR. FAYE: Well, let me just say that, first of all, the USGS, the mother and daughter of Modflow here, which is our simulator, they have exceedingly rigorous standards for qualifying their codes, number one. And typically, Dr. Johnson, the way this is done, they -- you recognize a standard groundwater problem that can be solved analytically. And then you pose that problem to the numerical code and see -- and compare that result against the analytical results. And I can tell you that that was done with a great deal of rigor by the USGS, and the

results were highly successful.

DR. JOHNSON: I have a couple of administrative questions, Morris.

MR. MASLIA: Yes.

DR. JOHNSON: The panelists have provided a set of written comments, premeeting comments. My question is: Will these be made part of the public record?

MR. MASLIA: They will be in the -- in a refined -- and when I say "refined" -- grammar and otherwise --- as part of the report -- the report about the meeting summary. Our past experience has been, like in Dover Township, they were included as an appendix to the report.

DR. JOHNSON: This is going back to Dr. Singh's comment this morning about the transparency of all of this effort. It would seem quite meritorious to have these part of the public record, whether it's the record of this meeting or some other source. Does any panelist object to having his comments made part of that record? Do you want time to "correct your premeeting comments," knowing now that it looks like they'll be in the public record? You should be given that privilege.

DR. DOUGHERTY: I'd like the opportunity to go back and just check. I don't have a problem with the principle.

DR. JOHNSON: Okay.

MR. MASLIA: Well, what will happen, based on our modus operandi from the past is that a draft meeting summary report will come out with your comments in the appendix. And each panelist will be given a copy of that draft meeting summary to correct their comments, see if it's misquoted, or anything else through our contractor, Eastern Research Group. And then once they hear back from you -- yea or nay or change page so-and-so -- then that will become a final meeting summary report. And that will be published and, as Dr. Singh's asked, put on the Web as well.

DR. JOHNSON: Does ATSDR plan to provide an answer to each of these questions?

MR. MASLIA: As closely as we can. In other words, some of the questions were -- the same questions were asked by multiple panelists. That's what I'm trying to say. I have not thought out yet -- if you're asking me going down each comment, you know, Panelist No. 1, you know, has ten questions. Do we answer those specifically, then go to Panelist No. 2, even though there's a repetition -- may be a repetition.

DR. JOHNSON: All right. That's just an administrative detail, you know. It's called "ditto" or something like that.

MR. MASLIA: Right.

DR. JOHNSON: But do the panelists feel the need for having an agency response to what would strike me as rather seriously thought-through questions? What are your expectations? I don't want to push something forward that's not palpable.

DR. POMMERENK: For me, personally, if I see that my comment has been addressed in a follow-up report -- you know, this is obviously a draft. If the final has those questions addressed because, you know, some of the questions were simply due because I could not find the answer immediately. If they were addressed now, for example, that would be fine, but if it's something else, or...

DR. JOHNSON: But there's another group of people who might profit from a reply, and that's the public.

DR. POMMERENK: Yeah.

DR. JOHNSON: I mean, here's a serious question from Dr. Clark. Number 5, what kind of errors might be inherent in these assumptions? Should that be answered and made part of the public record?

DR. WALSKI: I think that as long as they have addressed the substance of the comments, I don't think it's really a good use of resources to be going through question by question. It seems like that's excessive. As long as they substantially respond, I think, and

incorporate it in the report, that would be satisfying to me.

DR. JOHNSON: Okay.

DR. DOUGHERTY: For myself, they weren't -- were not prepared for the expectation of a point-by-point response because they were prepared to inform the agency about some of the issues that were on my mind that would be useful to hear about here. They were to prompt discussion as opposed to elicit responses. There are some that, certainly, are in that other category, but I think we've heard many responses; not all, but many.

DR. JOHNSON: I would offer the opportunity at 2:30 when the public addresses us to make comments on that same subject. But I think you have a sense from the panel that it might be -- it might be an overreach to provide a kind of point-by-point response to their premeeting questions.

MR. MASLIA: I thank the panel for clarifying that. Tom, your point is well taken about agency resources in general, but I think there are some points specific, like the boundary issue. I think that's a specific answer or approach that we've discussed here. But others will be generalized, and as Peter said, if he sees it in the final report --

DR. POMMERENK: Yeah. I'll --

MR. MASLIA: That's sort of the approach that we used

in Dover Township. We used a similar set-up with several panels. And the final report did either allude directly to some issues that were brought up.

DR. POMMERENK: Yeah. Many of my questions were -- they're clarifications questions --

MR. MASLIA: Right.

DR. POMMERENK: -- where I was not clear --

MR. MASLIA: Right.

DR. POMMERENK: -- and you --

MR. MASLIA: We appreciate -- I appreciate another set of eyes or ten sets of eyes looking over our shoulders to help us see the light of day.

DR. JOHNSON: Well, thank you. Let's take a 15-minute break, and when we return, we will start with the specific issues and questions for discussion.

(Whereupon, a recess of approximately 11 minutes was taken.)

MR. MASLIA: One issue: For our working lunch tomorrow and -- we're going to this place called -- or not going to, but we're going to order several platters of Roly Poly sandwiches, which include anything from monster veggie, California turkey, roast chicken, and all that sort of stuff; a variety of that. And so what the ladies up -- well, there's Ann right there -- need to know by the end of this afternoon is how many people want to

participate in that. It's a volume thing. And the price is based on the volume of whatever we order so -- and then they'll -- based on that, then tomorrow morning, they'll pass around envelopes to everybody, and you can put your, you know, five or six bucks in there.

DR. DOUGHERTY: When we do that, do you want us to raise our -- just raise our hands and get a head count now?

MR. MASLIA: Well, this afternoon, maybe, sometimes -- I don't know if we're taking another quickie break or whatever at some point. Ann.

MISS WALKER: Tell me if you're not going to do it.

MR. MASLIA: Oh, well, that's -- who doesn't want to do it? And that includes any people in the audience and public as well.

DR. JOHNSON: Thank you.

MR. MASLIA: Okay?

MISS WALKER: Okay.

MR. MASLIA: Okay.

MISS WALKER: I don't see any no's, so we'll just count. And then tomorrow morning, you can see Joann and give her some money.

MR. MASLIA: Okay. It's all yours, Dr. Johnson.

DR. JOHNSON: Well, let's turn to the questions that the agency posed that are specific to the groundwater

presentation. As I count them, there are eight questions, and there may be others that arise during the course of a discussion.

First of all, based on groundwater-modeling results presented, what modifications, if any, should ATSDR make? Who wants to take the lead on answering that, as I look around the panel? Let me warn you, I teach, so I know how to pick them (laughter).

DR. DOUGHERTY: I'm in the front row.

DR. JOHNSON: I know when I see people hunkering over. Robert.

DR. CLARK: Okay. I guess one of the -- one of the questions I had goes back to the relative importance of the work that's being done now versus the other contamination sources in the system. And would it be better to devote some resources to understand the relative impact of that, particularly on the epidemiologic results, as opposed to spending a lot more resources in refining the existing model? And I'm not clear on that. I don't have a clear feeling. It's a very impressive technical effort, but I'm not sure that it gets us very far as far as understanding what the other sources might be and what the impact might be.

DR. JOHNSON: Eric.

DR. LABOLLE: Yeah. I would like to add to that.

it's not clear to me yet the role of the groundwater model in the whole simulation process. And what I mean by that is some of the discussions we've been having over lunch and such and looking at this time-line chronology that's presented here and I'm looking at when the Tarawa Terrace wastewater treatment plant came on-line and when it was closed down.

And it looks like, you know, the contamination from the various wells is mixed at a single point, and it would be useful, actually, to have some kind of discussion at some point -- maybe perhaps tomorrow or something -- on the ranges of concentrations within these different wells and how much we really gain with additional detail in the groundwater model.

So I think -- I think any recommendations should be preceded with some further understanding of its role and how much is going to be garnered from additional work in that regard.

DR. CLARK: Another variation on that, too, is the amount of resources that are available to do the study and how does it take away from other type -- other parts of the study, which might actually have more impact, more importance.

DR. JOHNSON: Morris; Bob; whomever.

MR. FAYE: The objective of the groundwater model --

flow model is to form the basis of a fate and transport analysis using numerical models that will ultimately result in a monthly value of concentration of contaminant; i.e., PCE at certain wellheads. I mean, that's from -- for the period -- was it 1968 to '85? That's the objective. I think that was clearly stated several times. Now, if that's not a tenable objective, it would be nice to know that in your opinion. But that is the objective.

DR. KONIKOW: Based on your groundwater modeling so far, you're really starting in 1978 or '79 --

MR. FAYE: Right.

DR. KONIKOW: -- and so what's -- how do you hope to cover the period back through 1968 or so --

MR. FAYE: Good --

DR. KONIKOW: -- when the epidemiological data is starting?

MR. FAYE: Good question. The reason we did the '78 to '94, as I said, was because that's when we had some water-level data that we could actually pay attention to. Probably between 1952 and 1978, we may have a grand total of two or three dozen water-level measurements in comparison. Okay?

We also only have discrete -- a discrete window for about, oh, six or seven years, periodic nonconsecutive years; a discrete window in terms of a published value of

the -- of the quantity of water, the total quantity of water treated. We have another half a dozen references for different years in that interim, relating to well capacities and what wells were operational. The well capacities do change with time.

The flip side of that is that for most of that period -- and certainly the USGS data there for the -- for the pumping information from '75 to '86 indicate that within plus or minus 10 percent of about -- of .95 MGD that the average annual rate doesn't change that much. And that's because Tarawa Terrace, the housing units, were occupied just about 100 percent all of the time, 90 to 100 percent all the time. So we shouldn't be looking for a lot of variation.

We do have enough data now with the additional information that Morris discussed a few minutes ago. We do have enough data now, I believe, to make some sense out of monthly variations and pumping over a long period. And we can apply that information backwards in time as well. And that's kind of the summary of the suite of information that we have available to us, Lenny.

DR. KONIKOW: As far as exposure goes, though, there's no --

MR. FAYE: No. That -- that's historical reconstruction. I mean, that's -- we do know -- we do

know the -- within a year of the beginning of operations of ABC Cleaners, we know that they used only PCE during their whole period of operation. That's it.

MR. MASLIA: Based on suggestions also -- and this gets into, I think, Bob's question about resources and staffing. But, actually, I think another part of our effort or a more intense effort will be on data discovery. That appears to be a key factor, and I think going back to, like, tax records, maybe trying to refine the actual use of the PCE at ABC Cleaners.

And that calls into, as far as an answer in terms of agency resources, that's a two-part answer, and I think you can appreciate this being a former government employee yourself. As far as the, how shall I say, funding-part issue, I believe the funds are there. Okay. They've been there this past year while we've been doing fieldwork and that. The other side of the equation is the staff of personnel. That is not there. Issues of do we have enough staff -- and let me get into that.

As we discussed at lunch, unlike with other state programs that ATSDR has, we have no cooperative agreement with the state of North Carolina. We used that very heavily in Dover Township, New Jersey being a state. So that alleviated the need if we needed people to go and do some historical record search or do some detailed sitting

on site, so to speak. We actually had a field office over there.

So that assisted us. We don't have that option in this situation. So that means if I want to spend the next month, which maybe I'm just taking a month out of my hat, and doing "data discovery," going into the files at Camp Lejeune or something, somehow our project has to come up with a warm body to do that.

So while the funding may be there, the people are not there. And that's a consideration, I think, with recommendations, obviously, from the technical staff that management may need to look at that. If we say it appears to be a consensus of the panel -- I haven't taken a vote. That's -- Dr. Johnson probably will try to do that later on.

But if data discovery, refining our chronology, our operational history, and things of that nature to pinpoint specific lack of information that we have now is a -- should be a focus of our continued effort, then that's something we have to address, I think, as a division, as an agency. So hopefully, that's addressed your question. Is there a follow-up, or is there...

DR. JOHNSON: Well, what I hear is a strong commitment on the part of the agency to continue the groundwater modeling and activities associated with that

effort. I also am hearing from the panel some concern as to whether that, perhaps, the depth of that should be pursued. Am I misstating the case here? Please, Tom.

DR. WALSKI: What I would want to do as a starting point would just sort of do an overall classification of which areas we know were contaminated with this chemical, which ones we know were safe, and then which ones were -- and those you just sort of say, you know, these people were exposed, period, and these people were not exposed, and concentrate the modeling on areas that we're gray on. Do we have a marker for this easel here?

MR. MASLIA: I've got -- these are drawing markers, but you can --

DR. WALSKI: Here. Oh, here's one. Okay. How am I going to operate this thing? Okay. There we go.

(Drawing) It's sort of a thing like this with, you know, Terrace, Hadnot, Holcomb, 1952, 1972. You know, I have separate rows. 1971, 1987, and just draw these in. This one here is a -- this area where we know was bad here, we know it's cleaned up here because they shut the plant down, and we know that ABC Cleaners wasn't in existence before some date, possibly. So this we know, and we just want to focus the modeling in here to areas we're not sure.

And like, Holcomb, we knew was pretty good most of

its life, but there are some periods where we were uncertain of. And this might be where you'd look at some modeling where it was unsure. And Hadnot, we know was pretty bad throughout all time and you know, until they went to some type of -- what ended the -- they put some more treatment in, right, some pump and treat?

MR. FAYE: No, they didn't. They just took the wells off-line.

DR. WALSKI: They took the bad wells off-line at some point. So we know that after this point you're okay. But here we were in pretty bad shape. And then just focus in on the places where the models could tell you, you know, where it's critical because here you knew there was exposure. So you might want to do some kind of matrix like this as the next step before you got into, you know, doing -- just trying to model every single month of this thing where you know there's contamination in some of these areas. So why bother beating that when -- or you know that some of these weren't contaminated at that time, so why bother modeling those periods?

MR. MASLIA: My -- I guess, at least, my experience and knowledge would be in a numerical model, such as Modflow or any of its varieties. We have to step through time. So we're going to have to time step whether we -- whether we use the information or not, we're still going

to have to time step it to get to the period of interest. Is that --

MR. FAYE: And also, in terms of the periods of time when no exposure was occurring, your point's well taken. But it would be so much more convenient -- say, for example, we know that Tarawa Terrace -- I mean, the ABC Cleaners, for example, they probably started operations around 1955. We know that. And the Tarawa Terrace wells went on-line in 1952. From a modeling standpoint, it would be so much better to start your -- to start your simulation in 1952 because you're starting out from a prepumping condition, rather than begin things in 1955 and try to guess at what the antecedent conditions were.

So, you know, that's a decent trade-off. Three years is not a big deal. And we wouldn't have to do that, say, for example, on a monthly basis; those three years. So I think -- in certain context, I think your comments have a lot of merit. In that particular case, I'm not sure.

DR. UBER: I think that I'm taking Tom's comments as more metaphorically, maybe, not exactly literally, on that -- on that matrix. Just to -- what I hear some of the panel saying is that we might like to hear the objectives of the groundwater modeling explained more in the context of the ultimate goal of the investigation, meaning the epidemiological study and the needs for that.

So, for example, if you knew that these nine wells -- I'm not saying this is the case. But if you knew that these nine wells were all blended together and served Tarawa Terrace residents during a certain period, then that means that the groundwater model is really predicting the blended sum of those waters from those nine wells. And the -- and if you do sensitivity analysis, such that it doesn't really affect very much the blended water over time from those wells, then you -- you know, if that's the case, if that's insensitive to those assumptions, then those assumptions are not necessary to nail down any further.

Whereas, those same assumptions might have impacted significantly the individual arrival times at certain wells or individual captures zones. So, I mean, that's just an example. I'm not saying -- you don't need to comment on that particularly. But if that were the case, then that would be one example of making the objectives of the groundwater modeling, in my mind, closer to the needs of the epidemiological study because it brings it into the context of the exposure. Does that make any sense? I'm thinking not.

MR. FAYE: Yes, it does. The fly -- well, yes, your comments do make a lot of sense. The situation as it exists is that the results of the groundwater-flow model,

which would provide monthly concentrations at the wellheads -- those are one step removed from the exposure at a street or a house in the -- in Tarawa Terrace because that -- those results are linked to the network, to EPANET, to the network model, which provide the exposures at the individual residences or streets or whatever.

DR. UBER: Mm-hmm.

MR. FAYE: So the results of the groundwater flow model are one step removed from where you're getting to. But that's the linkage that the network -- the network analysis is the linkage.

DR. LABOLLE: So expanding on that -- Eric LaBolle here -- if one looks at the groundwater model and its results today, even though they're still in preliminary stages, can you make an assessment that some of these wells saw contamination for all time, for all the entire study period?

MR. FAYE: That's a really good point, and I was hoping somebody would ask that. My gut feeling right now -- and I could be wrong. But my gut feeling right now is that TT-26 is the major player in the whole -- in the whole event from the time that there was a breakthrough at TT-26 of the PCE from ABC Cleaners until the times that the wells were shut down. I think most of the PCE produced at ABC was captured at -- only at TT-26 with

maybe some residual amounts at TT-25.

There were -- we have that migration to the northwest. That was probably caused by local pumping there that we know nothing about as well as dispersion. But for all intents and purposes, the capture of PCE occurred at TT-26, and I think, you know, that that's going to be the end result.

DR. LABOLLE: And is it -- can you state an opinion at this point in time as to a range of times that you think the contamination might have arrived at TT-26? Not to pin you down, but my point here is this. My point is: If you're dealing with a study period in which TT-26 saw contamination during the whole time, that might change the role of the groundwater model versus a study period in which the groundwater model is expected to predict an arrival curve to TT-26. The level of detail necessary to predict an arrival curve would be significantly different than one needed to predict, say, maybe just a boundary range of concentrations --

MR. FAYE: Yep.

DR. LABOLLE: -- in which assumes --

MR. FAYE: Yes. That's good.

DR. LABOLLE: -- inherent uncertainty.

MR. FAYE: Yeah. That's very good. You have -- you have several issues to address, okay, in that whole

context. If you have the arrival time -- I made an estimate with the advective transport simulation. It occurred about -- in about three years. Okay. So if we assume that PCE entered the -- got -- was actually being discharged to the septic tank at ABC Cleaners some time in 1955, probably made it to the water table maybe a few months or a year later, you're looking at something around 1959 when PCE started to -- and that's not accounting for dispersion. It might have gotten there earlier when dispersion effects are taken into account.

Now, having said that, you have these other issues of retardation, biodegradation, and whatever that are going on in that interim -- in that whole period of time, say, from 1959 or whenever up to 1985 when that particular well was shut down and taken off the -- taken out of the network.

So what the model would be attempting to do, okay, would be to address those issues of retardation, dispersion, biodegradation, whatever, decay; and in that interim period of time for that particular -- for that interval.

DR. LABOLLE: The sense that I'm getting then is that the 15 years roughly -- or, say, 10 to 15 years that have elapsed there between the introduction of a source to the system and/or probable introduction of a source to the

system and the beginning of the study period sounds like sufficient time for the contamination to have arrived --

MR. FAYE: Oh, yeah.

DR. LABOLLE: -- at TT-26.

MR. FAYE: Oh, yeah; absolutely; absolutely. We would not begin -- or at least I would not think it would be appropriate to begin the model simulation -- the groundwater flow and fate and transport simulations in 1965, which is the beginning of the period of interest to the epi study. We would want to be there before. We would be simulating conditions before that and then all the way through it.

MR. MASLIA: One other issue because Bob and I have discussed this, and that's the issue of Well TT-23. And that, again, I think this is where the model can help refine our understanding. Well TT-23 was drilled after the shutdown or in anticipation of the shutdown of TT-26.

MR. FAYE: No. It was '84. Well TT -- we have a -- we have an actual step-drawdown test for TT-23. I think it was in March of '83. So TT-23 was sitting there available. That was part of Tarawa Terrace's routine operation of bringing a new well on-line and probably taking an older well that had reduced yield off of line.

And then all of a sudden, when they did the sampling while TT -- there was PCE that showed up in TT-23. So PCE

-- TT-23 never got a chance to be in operation probably for more than a year. But -- and frankly, I don't know how much importance the contribution of TT-23 had to the -- to exposure because it was only operated for such a short period of time.

But I will say that it's been on everybody's radar screen as a point of interest, and I do believe that the only way you're really going to understand whatever the contribution was from TT-23, if it remains a major point of interest as it seems to be, would be through a -- through numerical simulation.

DR. JOHNSON: Well, I think we've had a good discussion and some suggestions as to how the modeling work might be modified. It's certainly for the agency's consideration and final determination. But some interesting ideas were placed on the table, and we would ask that they be seriously considered by the agency.

As an aside, I have not forgotten about the public session, and I plan to do that at 3:30. So those of you who wish to speak at 3:30, be prepared to do so. We will need your name, et cetera. To the extent possible, focus on what we're discussing today: the water-modeling issues. But anyway, at 3:30, we will do that.

Let's continue on to Question 2, and, again, we can come back to any of these questions. I'm just trying to

get us through these series of significant issues. Number 2: Should ATSDR use the same level of detail; i.e., 50-foot cells and expand the groundwater model to include the Holcomb Boulevard and the Hadnot Point areas? If so, what level of increase in effort does the panel envision for this effort? Lenny, please.

DR. KONIKOW: Well, a 50-foot grid spacing seems, you know, reasonable, but I think the approach that, you know, I would recommend and probably other people would recommend is do some grid-sensitivity testing. I heard someone mention that this morning. Try a 100-foot cell, and see if there's any difference. Try a 25-foot cell spacing, and see if there's any difference. If it doesn't make any difference, stick with the 100 foot.

UNIDENTIFIED SPEAKER: Right.

DR. KONIKOW: If it makes a difference, depending on the nature of the difference, you probably want to go to the finer grid spacing. So it's hard to say if 50-foot spacing is the right one without looking at some sensitivity tests. So somewhere along the line -- and, again, this is one of the nice things about a graphical preprocessor based on a GIS-type system is that you can very easily change your grid spacing. And that's one of the things we'd certainly recommend doing.

As far as expanding it to the Holcomb Boulevard and

Hadnot Point areas, I think it depends do you want to apply a transport model there or not. Do you want to, you know, look at the -- I mean, you're starting in just the Tarawa Terrace because that's simpler. So if you can't succeed there, then maybe there's no point going to the other systems.

MR. FAYE: And that's -- that was the whole idea.

DR. KONIKOW: Yeah. So I think you have to kind of see what the results are after a little more time.

MR. FAYE: Good. Thank you.

DR. JOHNSON: Other comments on this question? Vijay.

DR. SINGH: I think you may also want to look at variable grid size. You may want to consider finer grids near the source and coarser away from the source.

MR. FAYE: Yeah. That's clearly -- that's clearly something that we intend to do. And as Lenny said, when you're using a GIS conditioner for your input arrays, why, it's really easy to do. It's not a problem, and that's something that we very definitely would look at or intend to look at.

DR. JOHNSON: Any sense on what extra level of effort would be required?

MR. FAYE: Not a whole lot.

DR. JOHNSON: I'm not sure -- I'm not sure that's the

kind of thing a panel is equipped to come to grips with, but I speak only for myself. I haven't a clue as to how efficiently you work and other -- what equipment you have.

MR. FAYE: My response was just to the specific notion of changing the grid dimensions. Okay? I mean, I didn't know you were touching on the overall issue.

DR. JOHNSON: It's part of the question.

MR. MASLIA: Let me just address this. The reason that question came up is looking at the, I guess, experience and expertise and different type of analyses that some of the panel members have been involved, I suppose we were looking at it based on their experience of saying, "Oh, no. That's going to take a completely separate project team. You know, that's going to take another three years, five years, or whatever based on our experience."

And that's something -- an input that we need and to discuss with the epidemiologists as whether that increase in effort is warranted for the type of results that we may obtain. It clearly has been referred to on a number of occasions now. If, in fact, we're having some difficulty, although maybe success, in Tarawa Terrace in this level of effort now, expanding that difficulty at least an order of magnitude because of uncertainty and unknown in Hadnot Point and the variety of nonpoint specific sources, that

may be an area that we may say that the level of effort will not warrant the refinement in the answers that we need for Hadnot Point area. And that's really why that was posed, not looking for a specific person number or hour -- labor hours or anything like that.

MR. FAYE: Could I say something?

MR. MASLIA: Yes.

MR. FAYE: With regard to the additional complexity that we're fairly certain that we would see at Hadnot Point, perhaps, an intermediate step or even a final step to simulating various concentrations at a great number of wells with numerous source areas would be analytical, rather than numerical, which would greatly simplify the situation in terms of analysis. But what would also be somewhat limiting in terms of the results that we would provide -- be able to provide for the epidemiological study. But it may be a very useful intermediate step.

DR. JOHNSON: Yes.

DR. CLARK: The answer -- it seems like the answer to this question somewhat answers the concerns I had on the first question. In effect, what you're doing with Tarawa Terrace, that's basically a pilot study to validate, develop groundwater-transport model; right?

MR. FAYE: It's -- I would say it's perhaps a little further than a pilot study. We know that these things

have been done before. There's not a lot of mystery about it. More the issue is, yeah, we can do it, and we can give you an answer, but just how damn good is the answer? Okay?

DR. CLARK: And so if you have success at Tarawa Terrace, then the potential for applying it to other areas increases, I suppose, significantly.

MR. FAYE: Yes; sure.

DR. CLARK: And so that basically is kind of the reason that you're taking that approach on the project.

DR. JOHNSON: Yes, please.

DR. UBER: Could I just follow up on that real quick? Could you clarify for me: Is the proposal -- I know we're talking about just the groundwater analysis now. But is the proposal to use Tarawa Terrace really, truly as an advanced pilot study but moving it from the groundwater to the water distribution through to the epidemiological conclusions prior to moving significantly or changing directions drastically for some of the other areas?

MR. FAYE: That's yours, Ace.

MR. MASLIA: That is -- our intent is to hopefully -- I don't want to say wrap it up -- but put some finality on our state of knowledge and conclusions we can make from the effort at Tarawa Terrace in terms of the groundwater fate and transport and the distribution side. That is the

-- as we've alluded to, we know we've got one primary well, TT-26. We've got some data gaps in historical or chronology.

But as far as the hydrogeologic framework, we've defined that as far as modeling. When I say "boundaries," not the physical model of the boundaries, but where we should start our timing, stuff like that. We've got -- we're getting more well-production records. As I said, we just got some more in the middle eighties to fill in some gaps. So that's pretty much further along. I can't speak as far as the cases and controls. Dr. Bove can probably speak more on that if he thinks it's appropriate to discuss that issue.

MR. FAYE: And there's also another major issue implicit in that -- in that question. And that is the actual linkage between the models. The results of the groundwater flow model I used as input into EPANET or some similar thing. And we want that to be as transparent and as fluid as -- no pun intended -- as fluid as possible. We don't want that to be a stop-and-start, really hard-nose mechanical-type of operation. And so there's some issues there to be dealt with in terms of refining that.

DR. UBER: So that's good. That actually reinforces the point, perhaps, of making a decision to try to do it all with Tarawa Terrace. It sounds to me like maybe the

team is not quite committed to doing that because there's some, maybe, uncertainty, reasonably, about the time frames of the, you know, getting all the control group together and doing all of that work.

But I -- personally, I would be very much in favor of that approach, if it is feasible at all, because I think, you know, well, you always learn from doing it. And I think bringing this -- bringing that study to the end conclusion, even on a first-order basis -- end, meaning to some kind of integration with the epidemiological conclusions -- would be a good thing to add going into the other areas.

MR. MASLIA: The other thing, if I might just jump the gun for either this afternoon or tomorrow's presentation on the water-distribution side, I alluded to earlier in my opening remarks that we do have an analysis. Claudia did a very good analysis on building use and building type and, you know, whether it's residential, family housing, industrial, car wash, and so on. And I'll show that later on either tomorrow or this afternoon, depending on the time.

But what you will notice is obviously Tarawa Terrace is 90-plus percent family housing. Holcomb Boulevard is 90-plus percent family housing with elementary schools and high schools. When you get down to Hadnot Point, it's

just the opposite. It's 90 percent plus industrial and other things and bachelor housing with maybe 5 percent family housing. Would that be about right, Claudia, somewhere around that?

MISS VALENZUELA: Yeah.

MR. MASLIA: Yeah; about like that. So that's the other -- we haven't gotten into that, but you'll see some maps on that. So that's the other consideration really from our standpoint.

DR. WALSKI: When the distribution system measurements for PCE were made in Tarawa Terrace, what was the range of values at the tap?

MR. MASLIA: PCE or TCE?

DR. WALSKI: PCE at Tarawa Terrace, like, the range. Was it a huge range? Did it show tremendous variability, or was it basically, once you got it, you got it?

MR. MASLIA: We've got a map with the chronology on them.

MR. FAYE: Yeah.

MR. MASLIA: Here. We've got a chronology here. Here we go. Actually --

MR. FAYE: The concentrations at the tap were probably somewhat less to greatly less variable than the concentrations that we observed at the wellheads.

DR. WALSKI: Because everything gets blended, and

so --

MR. FAYE: Right.

DR. WALSKI: -- it seems like, basically, once the system gets contaminated water in it, the people get contaminated water, and, you know, the amount that the model is going to tell you is, well, maybe they got 52 instead of 54. But the fact is that once the plume hits the wells and they use the wells, everybody got the same thing in that system. That, you know, I'm just questioning how much more you're going to get by really refining the models.

MR. FAYE: Don't know; don't know. I can't -- I couldn't -- I know that the concentrations at the wellheads vary by orders of -- by an order of magnitude at least. And I'm not -- I'm not sure that I'd be comfortable in going into detail even about a cause and effect of that. I don't know that. I haven't reasoned that out that well. I just -- that's it. I -- you know, that's the extent of the information.

DR. LABOLLE: Particularly with regards to the distribution system model, I think that what's been raised here is quite important. If you're putting in a source and everybody has to drink that water because there's only one source in the system, which is the wastewater treatment plant, at least during a significant portion of

the time, if not all the time in the study period, then how does refining the model increase one's information on exposure?

MR. FAYE: Well, for one thing, when we -- when we finally get to the point where we're able to deal with monthly recharge and we have some decent confidence that we're doing a good job there, you're looking at -- you're looking at orders of magnitude change and recharge from month to month. Okay?

DR. LABOLLE: My question was with regards to the distribution-system model though.

MR. FAYE: Oh, I'm sorry.

DR. LABOLLE: But I have one for the groundwater too.

MR. MASLIA: Let me -- if we assume that you've got several wells and they're all blended in at the treatment plant and then they go out into the distribution system and are up in the tanks and equally mixed and all that, then your point is everybody gets the same blended concentration of water; no question about that.

We found a couple of things, and again, this is probably something we'll get into tomorrow or this afternoon. But we are finding, at least in the storage tanks, that it's not a complete mixed situation. This is based on some field testing that we did this past year.

We're not sure if you're seeing last-in/first-out or

a compartmental-type issue in the tanks. We're testing that out, doing some sensitivity runs right now, so that, if you had in one given month one well running more than the other, either contaminated or not contaminated, and pushing that out through the treatment plant and then stored up in the tanks or whatever, you may not necessarily see that water coming out into the distribution, depending what's going on in the mixing in the tanks.

DR. LABOLLE: Then in that case then, the study, you know, the detail would then focus on a very restricted portion of the system, that being the tank and one of the sources --

MR. MASLIA: Mm-hmm.

DR. LABOLLE: -- which wells the sources were coming from?

MR. MASLIA: That's correct.

DR. LABOLLE: But then the rest of the distribution system, the detail and the level of analysis would have little effect then on exposure. Am I missing something in that?

MR. MASLIA: Well, the only thing we're -- or we're trying to understand right now is we're still in the process, at least for present-day, trying to understand exactly how the tanks are mixing. We've instrumented some

tanks, and it's raised some additional questions. And I really can't, at this point, answer: Can we make some either simplifying assumptions or assume, given a certain input from the treatment plant, that this portion of the system received this slug of water or not?

I think, perhaps, maybe the panel will see some insights from some of the data, more detail that we'll present either this afternoon or later tomorrow. Those are some good issues to bring up.

DR. CLARK: Depending upon the variability on the input side, you could get blending in the system that would cause different levels of exposure to individual households too. So I guess it's those issues that you have to resolve.

DR. LABOLLE: Yeah. Particularly if the treatment plant doesn't. You know, the treatment plant is delivering water out into various pipes into the system at that point, then the detail -- I could see the distribution system would become important.

MR. FAYE: On the groundwater side, you would have an expectation of variability. We don't know how much. Depending on your rainfall, which would translate -- the way we're looking at recharge now would translate directly to recharge. You would have periods of time when you'd virtually have no recharge, probably extended periods of

time. And then you'd have other times when you would have just an excess of recharge.

How this affects the -- would affect the variability of concentrations at the wellhead, we just don't know. And it -- is that the reason of the order of magnitude change in contaminant concentrations at the various wells? We don't know. But we do know that there is a great deal of variability in concentrations at the wellhead, just based on observations.

DR. DOUGHERTY: I have one question for -- actually your comment and Eric's. Since you're preparing, planning to perform a fate and transport model --

MR. FAYE: Ultimately.

DR. DOUGHERTY: -- ultimately. And this is a question about your preliminary thinking, and so it's subject to draft and revision and all these things as the project evolves. But the question is: How do you think you're going to handle the source? How is it going to be represented?

MR. FAYE: Well, as Morris said, one thing that we have in the works is to use Dr. Aral's expertise at Georgia Tech. Are you familiar with CXTFIT?

DR. DOUGHERTY: Sure.

MR. FAYE: Okay. It's kind of a simplistic notion, but, you know, it's the same idea where you would actually

look at your observed concentrations in a "plume" and then be able to compute backwards and estimate a source concentration for a limited period of time relative to those observed conditions.

We have data in 1985 that probably -- early 1985, that probably represents, goodness, for want of a better term, routine operating conditions, okay, at the -- at ABC Cleaners. And we're looking at 12,000 micrograms per liter there. The gentleman earlier made the point that there may have been a greatly increased rate of input into the system during Vietnam.

And hopefully, hopefully, through the data discovery that Morris was talking about with the tax returns and whatever, we can get something of a handle on that. Obviously, it goes without saying, I mean, the source term is the -- is -- it's not all the eggs in the basket, but it's a good number of them.

DR. DOUGHERTY: My question in particular was: Is it going to be treated as a specified concentration, or is there going to be -- or are you anticipating a process model for --

MR. FAYE: No.

DR. DOUGHERTY: -- some dissolution process?

MR. FAYE: I -- that, we haven't thought of yet. My -- right now, my thinking would be basically just a rate

at a -- at some concentration. Okay?

DR. DOUGHERTY: Some of mass loadings?

MR. FAYE: Yeah; right.

DR. JOHNSON: Okay. Let's stop at that point. I think we've -- the panel's given you some excellent advice and some perhaps new directions to consider: grid sensitivity, testing, et cetera, other ideas. Again, we can always come back to any one of these questions.

The third question, before we have the questions from the public: Rather than developing three distinct groundwater-flow models, should ATSDR considering -- should consider developing one model?

DR. CLARK: It sounds like the answer to that has to be no, given the complexity of trying to do that.

DR. JOHNSON: The answer is no.

UNIDENTIFIED SPEAKERS: It may be later.

DR. DOUGHERTY: And then you have the choice of whether you do two and three or whether you expand one and two or incorporate two and three or whether it's a similar approach at that point.

DR. LABOLLE: Where does the third one come in? That's actually where I'm confused. We have Tarawa Terrace. We have Hadnot Point. It's my understanding that the community in the middle wasn't receiving much contamination; is that correct?

MR. MASLIA: Actually, correct, unless we find any other information to the contrary. That was probably a rush to write questions down, but I suppose one -- when I was thinking also of three models, one way I was thinking back to my USGS days is where you have an overall model and -- one model for the whole area, which may be a coarser grid, or define some boundary flows or whatever and then you have the two refined areas.

But from what our discussion this morning and this afternoon is going is, I believe, we'll be doing good to get at narrowing uncertainty or addressing uncertainty with the Tarawa Terrace area. I mean, I think there's some issues there that may, in fact, tell us, you know, don't go down the direction of the numerical model to Hadnot Point.

MR. FAYE: Accept no.

MR. MASLIA: What?

MR. FAYE: Accept no.

DR. JOHNSON: Okay. I think you got a clear answer on that one. We need to take about a five-minute pause or so, so that our recorder can recalibrate her recording equipment. And then after that, we look forward to comments from the public, and then we'll resume with the rest of the questions. So take a brief break of about five to ten minutes.

(Whereupon, a recess of approximately seven minutes was taken.)

DR. JOHNSON: We are at the point where we would be pleased to hear comments or observations from the public, and please come forward to the dais. Tell us your name. To the extent possible, we would ask that you summarize the significant points that you wish us to hear.

MR. ENSMINGER: Good afternoon.

DR. JOHNSON: Good afternoon.

MR. ENSMINGER: My name's Jerry Ensminger. I told you who I was earlier. I lost a child due to this contamination, and I have been deeply involved in this since 1997. Likewise, a retired major, Thomas Townsend, who I work very closely with and have worked with him for many years on this, and this following statement is a -- and questions is a combined effort between Mr. Townsend and I. And without further ado:

Construction of the Tarawa Terrace housing area commenced in 1952 and, at that time, was owned by Spangler Real Estate Company. My family lived at Tarawa Terrace, 3442 Hagaru Drive, from January 1955 to May of 1956, as cited in CLW-2982. In 1958, TT-26, the first of eight water supply wells, was constructed in Tarawa Terrace. The year 1961 saw the construction of an additional three wells: TT-52, 53, 54. Wells 27, 31, and 25 were

constructed in 1972, 1973, and 1980, respectively.

Well 23 was constructed in 1984. However, this well was never put on-line or in -- never put into production, as PCE was discovered immediately following construction. This well is also described as TT-NEW WELL in the same documents.

I provided you with a list of the supporting documents that support this statement. TT tap water was tested 27 May 1982 from seven wells less TT-23. PCE was found at 80 parts per billion and on 27 and 28 July '82 retested with PCE at 76 parts per billion, 82 parts per billion, and 104 parts per billion. TT wells were sampled in July of 1984; TT-23 at 37 parts per billion; TT-25, trace amounts; and TT-26 had 3.9 parts per billion. No TCE was detected.

Tap water in Tarawa Terrace was tested again on 5 February of 1985. The analysis indicated PCE at 80 parts per billion, TCE at 8.1 parts per billion, and DCE at 12 parts per billion. All Tarawa Terrace wells were disconnected from the water-distribution system on 8 February 1985, and Wells TT-23 and 26 were closed.

Four days later, on 12 February 1985, and again on 19 February of 1985, water from the TT system was tested and determined to contain no VOCs. Unable to meet the increasing water demand without these wells, the Tarawa

Terrace water-distribution system was supposedly closed. None of the TT well data, installation or operational date, and contamination testing results can be confirmed by this reporter since Marine Corps base Camp Lejeune has not provided same after many FOIA requests submitted; no responsive documents.

Question: If the TT water-distribution system was closed in February of 1985, where did the potable water to support some 1843 housing units and commercial establishments come from to fill that void?

DR. POMMERENK: Can I answer that question? I believe, in 1984, there was a pipeline constructed from the Holcomb Boulevard treatment plant, and that pipeline connected directly to the raw-water tank. So you received treated water from the Holcomb Boulevard area.

MR. ENSMINGER: In 1984?

DR. POMMERENK: I believe so. I would have to check the numbers, but that's the approximate time frame that I recall from the...

DR. JOHNSON: Come to a microphone, please.

MR. FAYE: The records that I'm familiar with that we've obtained from Camp Lejeune and other sources indicate that only Wells TT-23 and TT-26 were taken off-line in February of '85, that the other wells in the system at that time continued to operate, probably,

through all of '85. We know for sure that the water-treatment plant was operating and processing water at least up to March of 19 -- 1987. There's a real question about 1986. My gut feeling is that the ex-TT-23 and ex-TT-26 at the Tarawa Terrace wells probably operated all through 1986 as well.

Just with some corrections here to what this gentleman has said about TT-26, we have copies of notes from Mr. - a Mr. R. E. Peterson, who was an employee of the Lejeune facilities at that time in May of 1951, where he describes the construction and -- the drilling and construction of Well TT-26, TT-27 and 2-A. At that time, they were called Number 1 and Number 2-A and 2-B; 2-B was TT-27. So that's just a few comments there.

Thank you.

MR. ENSMINGER: And if you would, in your supporting documentation that I've provided you, CLW No. 1129 through 1131 was an action brief prepared by the Chief of Staff of Marine Corps base and is dated 1 March of 1985. That's Colonel M. G. Lilley, who I have spoken with personally. And he gave a -- his action brief was -- the subject was "Alternatives for Providing Water to Tarawa Terrace Area." So if a pipeline was installed in '84, why are they having an action brief in '85?

DR. POMMERENK: That's a good question. I was just

aware of the construction date of that pipeline. Obviously, my conclusion was that there was water supplied which may have not been started at that point.

MR. ENSMINGER: Well, while we're speaking about that, the next part of the question: Action brief for the commanding general of 1 March 1985, which I just referred you to, had seven alternatives, ranging from hauling water in tankers or construct a new 8-inch line from the Holcomb Boulevard water-treatment plant, which was being upgraded from 2 million gallons to a 5 million gallon per day capacity, or turn on the contaminated wells that have been shut down if required to maintain adequate water levels; estimated cost: none. New water -- new line was installed, temporary auxiliary line, in June of 1985 from Holcomb Boulevard water-treatment plant to the TT distribution center.

Question: Definitive criteria for describing -- describing operation of well status at Marine Corps base is confusing by using active, inactive, closed, abandoned, on-line, off-line, et cetera. CLW-2963, which you have there in your references, wells are taken off-line or out of service for short periods for maintenance; pumps are replaced; screens are cleaned; new data loggers installed.

Too many reports from Marine Corps base will show X-well closed in 1965, then in operation again in 1967, shut

down in 1968, operational in 1969. Having run water systems, I consider a sequential pattern: One, electricity turned off, pump in well, et cetera, et cetera.

Wells are either on-line or off-line; active or inactive; temporary nonfunctioning for service or long-term nonfunctioning, which can show as permanent non-serviceable; to be abandoned. Is there a sanctioned set of rules -- state, federal, American Water Works -- that can demystify this melange of terms, which are chaotic, at Marine Corps base?

DR. JOHNSON: Does anyone know?

(No audible response)

DR. WALSKI: Well, unfortunately, I think the terminology is whatever the person who wrote it down felt like writing that day. That's unfortunately the case.

MR. ENSMINGER: And another thing is, especially over in the Hadnot Point system, when you look at the Marine Corps' chronology, you would find wells that were taken off-line for contamination. And later on in the events, you'll see that it was taken off-line again for contamination, which tells me it was back on-line.

DR. POMMERENK: I guess the only state regulation, current state regulation, in North Carolina that I recall that would relate to that is that you have to, I think, file a record of abandoning a well if you take it

completely out of service. But otherwise, I wouldn't know of any, you know, regulatory issues regarding this terminology.

The other issue that you just addressed, and I'm just -- one problem could be -- and we have observed it in Camp Lejeune -- that sometimes a new well is drilled and it receives the same well number as the old well. That may have not happened in Tarawa Terrace, but I'm just throwing this out as a thought.

MR. ENSMINGER: You said at Lejeune there were wells -- new wells that were drilled that had the same number as the old one?

DR. POMMERENK: Yes. This has happened.

MR. ENSMINGER: Where?

DR. POMMERENK: I can't cite the exact numbers.

MR. ENSMINGER: Which well numbers?

MR. FAYE: Peter, I think, you know, your statement may be only partially correct. What happens in the -- when the contract -- at least as far as the documents that we have, when Lejeune turns loose of a contract, either for bidding or whatever, they'll -- there's a note on that "Well Replaced." Okay? And the old well number goes in there because there is no new well yet. Okay?

And so what happens then is the driller comes along and creates that suite of documents, like the drillers'

log or Elog or whatever. And they'll put in new HP-645 or something like that or new HP-647, which is what you're referring to. But that number, in my experience -- and I've looked through dozens of these records -- that number doesn't actually stay in the system. Okay? That new something or other gets a new number. Okay? Ultimately, as far as I can tell from the Camp Lejeune records, that well gets a new number. It doesn't -- it doesn't stay the old number very long.

DR. POMMERENK: Okay.

MR. FAYE: Okay?

DR. JOHNSON: Please proceed.

MR. ENSMINGER: When were the wells or the eight wells at Tarawa Terrace taken 100 percent out of service and abandoned? When were they taken out? When were they absolutely abandoned, closed, pumps pulled?

MR. FAYE: May I address that?

DR. JOHNSON: Would you stay up there, please.

MR. FAYE: I think that's a really critical, critical, critical question. The only -- what I can say with relative certainty is that TT-26 and TT-23 were removed completely from service in February of 1985. We have records in January and February and March of 1987 that indicate that the Tarawa Terrace -- and also, I think if you look at the plant capacities, you would really have

some bit of difficulty believing that Holcomb Boulevard could supply all of its needs, its original service area needs, and Tarawa Terrace needs during 1985 and 1986. Okay?

Maybe it could, but I think there would be some real serious operational difficulties. Unfortunately, the records that we have, like, for example, for monthly discharge -- monthly water-treatment plant operational records that give flows for a particular month that are exceedingly complete from 1980 to 1984 and then again exceedingly complete from 1987 to 1989. For some reason, these records for 1985 and '86 have just up and disappeared. No one seems to know what happened to them, but I believe they certainly existed.

My own feeling, as I expressed a few minutes ago, is that ex-TT-23 and ex-TT-26, the remaining wells at Tarawa Terrace that were operational in 1984, probably continued -- most of them -- in operation in 1985 and 1986. But we really -- and we do know that something was going on at the WTP in early 1987. But we really cannot say what was going on with the wells, what the well operations were in '85 or '86. The records for that period of time have just fallen into a black hole somewhere.

DR. JOHNSON: Okay. Let's continue. I'm going to ask ATSDR to provide answers expressly to each of these

questions. I don't think that's an imposition on the agency. To the extent that we can provide some feedback today, we will try to do that. But if you're looking for complete, satisfying answers, this isn't -- this isn't the forum for that. But please continue.

MR. ENSMINGER: Well, in response to what Mr. Faye just said, you have there in your package CLW-1914, which is a handwritten memorandum and it's dated in 1991. And it stated in this handwritten memorandum that TT-23, TT-25, and 26 has pump, will run. However, the well was closed. I mean, they weren't 100 percent decapacitated.

MR. FAYE: That's a note from, I believe, Daniel Sharp, from the facilities branch at Camp Lejeune. And that was written in a -- in specific -- as a specific response -- as a request from either EPA or Weston Engineers as they were preparing the Operational Unit 1 project to study the contamination caused by ABC Cleaners. That was a note to Camp Lejeune and a response, asking which wells were operational so that they could prepare to sample them.

MR. ENSMINGER: Well, there are means of pulling the pumps and putting a -- and still taking samples.

UNIDENTIFIED SPEAKER: But it may be more convenient.

MR. ENSMINGER: Okay. All right. If the TT well fields were not incapacitated in 1985 and an auxiliary

line to Tarawa Terrace, back and forth from Tarawa Terrace to Holcomb Boulevard, was in place in June of 1985, how do we know if Holcomb Boulevard water-treatment plant did not receive raw water from the Tarawa Terrace well fields?

MR. FAYE: We don't, and we actually have just the opposite information, a report from Geophex -- was it 1991, Morris? There is a -- there is a consultant's report that we have that we've recently referenced from a firm called Geophex out of Raleigh, North Carolina, that indicates just what Mr. Ensminger has said, that indeed, perhaps in 1989, the Tarawa Terrace wells were used to supplement the water supply to the Holcomb Boulevard water-treatment plant and perhaps for even an extended period of time in that -- within that year or maybe several years.

DR. DOUGHERTY: Did you say '89?

MR. FAYE: Yeah.

DR. WALSKI: But wouldn't they have to construct another line to go across, then, a raw-water line because you can't send the raw water over and treated water back in the same pipes. So they had to put in another line, so there'd be some record of that.

MR. FAYE: Yeah. One of those -- the report continues to say that whatever those operations were, Tom, that they ended when the -- when a freeze occurred and the

pipe collapsed into Northeast Creek. So whatever was happening there, it ended when the pipe collapsed. Okay? But I agree with you, and perhaps, there were dual pipes there. But we don't have the details.

MR. MASLIA: Let me just, if I may, qualify that again in terms of data discovery and all that. We just came across this report, actually, a couple of weeks ago, maybe less than that. It's a report that's dated 199 -- March of 1991. And on page 23 it makes the specific -- apparently the author of the report, who we're trying to find out still who the author is, makes the statement going over historical issues with different well fields, and it talks about the Tarawa Terrace well field.

And it says two years ago, which would make it '89, that the Tarawa Terrace wells supplied Holcomb Boulevard with water. That's almost a verbatim quote. I've got the report with me. I have called the Geophex office in Raleigh. They are no longer doing environmental report, and I'm on my third contact, trying to actually pinpoint -- if I can pinpoint the author of the report, as well as we've asked -- we do have a contract number, Camp Lejeune contract number, for that particular report. And we have asked and I think the folks from Camp Lejeune are preparing some documents for us on the entire contract that generated that report. So we may find out more

details, but that's what we have that's come to our attention within the last couple of weeks.

MR. ENSMINGER: If you'll take a look at the 1 March 1985 action brief by the Chief of Staff, Colonel Lilley, go to the last page, which is 1131. Please note under advantages, Item No. 5: Potential future use to return raw water from Tarawa Terrace wells. And I'd like you to look at Number 2 as well: Availability of water. Can draw from Holcomb Boulevard and Hadnot Point system, which leads me to believe that that interconnecting valve between the Holcomb Boulevard system and the Hadnot Point system was being opened, just by that statement in Item No. 2.

DR. JOHNSON: Any reaction, Bob or Morris?

MR. FAYE: That could easily be a --

MR. MASLIA: I'll only address one of the issues that has been brought to our attention previously, and this is by a different -- a congressionally mandated panel that occurred what? In February, Frank? Yeah, in February.

And we were repeatedly -- I was repeatedly asked the question: Would we and could we model the interconnection? Because, again, the understanding or the statements have been made previous to our investigation that the interconnection was only for emergency purposes, meaning, you know, neither short supply and by definition emergency -- and we've had this discussion with the present-day

operators of Camp Lejeune -- would be, you know, a day -- maybe a day or two if either something broke or needed extra supply of water.

That panel specifically wanted to know if we could model, you know, several weeks to several months at a time of interconnection on that. And my answer to them, just to complete the answer, would be that's where we would need distribution-system models to model that interconnection.

DR. CLARK: It sounds like Tom's point --

COURT REPORTER: Microphone, please.

DR. JOHNSON: Use the microphone, please.

DR. CLARK: I'm sorry. Could we turn -- it sounds like this pipe was designed to do both things: potentially, to return raw water from Tarawa Terrace as well as provide treated water from Holcomb Boulevard and Hadnot, which is very unusual to do that.

MR. FAYE: Don't forget now, you're dealing with two pipes, okay, one connecting Tarawa Terrace and Holcomb Boulevard and the other connecting Holcomb Boulevard and Hadnot Point.

DR. CLARK: Yeah. But this talks out -- oh, I'm sorry. Yeah. This talks about one pipe: construct 8-inch line from Brewster Boulevard to Tarawa Terrace. And then it has advantages, and I assume that refers to the --

MR. FAYE: That's --

DR. CLARK: -- 8-inch line.

MR. FAYE: That's the one from -- that's the one that apparently froze up and fell into Northeast Creek.

DR. CLARK: Okay.

MR. FAYE: If they actually built it, which we don't know.

DR. CLARK: But they're talking about a potential use of both supplying raw water as well as --

MR. FAYE: That refers to what Tom was talking about.

DR. CLARK: That was Tom's point.

COURT REPORTER: You need to be at the microphone.

UNIDENTIFIED SPEAKER: Sorry.

DR. JOHNSON: Okay. Shall we move along?

MR. ENSMINGER: All right. How do historical water-system operations, assession, monitoring, treating, and distribution at Camp Lejeune relate to systems of comparable size of population served during the same general time frame from 1950 to 1985 in the United States' civilian world? In other words, how does -- did the operation of Camp Lejeune and presently how does it stack up against its civilian counterparts?

MR. MASLIA: Could I give you a brief answer now, and then, since we haven't got into the distribution side of

things, give you a more detailed answer tomorrow? Because I do want to answer that, so -- but I didn't want to go off on a --

MR. ENSMINGER: No.

MR. MASLIA: -- tangent right now, if that's okay with the Chair.

Briefly, based on our experience, it's -- and I'm talking about Camp Lejeune, not other military installations, but it's night and day. There's almost basically an intent to make it demand independent; in other words, so they maintain constant pressure, constant level in the tanks.

They don't empty the tanks out, as opposed to, say, our work where we saw in Dover Township where there's more of a sinusoidal, a filling of a tank during periods of low demand, you know, midnight through four a.m. and then using that supply of water in the tanks and draining it out as people take showers or restaurants come on.

At Camp Lejeune -- and I'll admit our understanding still is not complete as total operation -- even for present day, we still have questions. They basically almost maintain a constant pressure, maintain a constant level in the tanks with the exception of one controlling tank per service area. And based on the water level in that controlling tank, which, based on our present-day

information, may only fluctuate from a -- from half a foot to maybe 6 feet at most. It's in a paper we prepared. That's the maximum fluctuation we have -- we have seen based on data for present day.

Then trigger high-lift pumps to turn on, say, at Tarawa Terrace to push water through the system. So it is a totally different way of operating, and that's one of the lacking pieces of information is specific diurnal demand. You know, the military personnel, enlisted people, you know, may get up at four or three a.m., and that's when, maybe, your maximum use may be. And then it may trail off six, seven a.m.; whereas in a more urban setting, like Dover Township, you may not see a peak in demand until eight -- seven or eight o'clock in the morning. And then it levels off, and then another peak at six p.m. when people come home. And we're still trying to understand it, but typically it's a vastly different way of operating.

DR. CLARK: But they do -- they do meet the requirements of the Safe Drinking Water Act. I think that's a commitment on the part of the military to do that.

MR. MASLIA: Oh, I wasn't referring to Safe Drinking Water Act.

DR. CLARK: But in terms of treating water, they meet the requirements of the Safe Drinking Water Act.

MR. MASLIA: Right.

DR. WALSKI: Yeah. I wouldn't say "night and day" either. I mean, there's a wide range in the way systems are operated around the country, and they're somewhere in the band. You know, they're more conservative though. From what I've been reading here, they're more conservative. Like, they try to keep raw water in storage for fires and emergencies than the average system, which allows more fluctuation.

MR. MASLIA: Yes.

DR. WALSKI: But it's -- so they're a little more on that side of the curve. But there's a wide range of operations. If you go -- every time I say I've seen it all, I go to the next water system. I see something totally different.

DR. CLARK: That includes civilian water systems too; right?

DR. WALSKI: Yeah; civilian and military.

MR. FAYE: I don't -- I don't mean to belabor the situation, but it is really important. Going back to the use of the wells at Tarawa Terrace during 1985 and '86, we do know that from Naval records that water samples, specifically to identify any contaminants, were collected

at the water-treatment plant at Tarawa Terrace weekly from March 1986 to March 1987, which certainly lends [sic] me to believe that -- that the wells were operating during that period.

And monthly samples were collected at TT-25 during that same period, so there was this continuing concern on the -- and these -- this sampling program was recommended by North Carolina DEM and, I believe, implemented by the Navy, by the Marine Corps.

So it just seems rather incongruous, if the wells were not operating and if there was still a not a concern about contamination, that none of this sampling program would have been implemented. And that's the main reason that I believe that the Tarawa Terrace supply Wells ex-TT-23 and ex-TT-26 were operating during 1985 and 1986.

MR. ENSMINGER: I know that flow meters have been installed during the conduct of this study. It's been published in the newspapers down at Camp Lejeune. What results can be made public at this time, and do they -- do they match your expectations?

MR. MASLIA: Again, we'll get into the specifics this afternoon and tomorrow, but basically, flow meters were recommended -- or requested to be installed by ATSDR because we could not just, based on system records

available to us, get a handle on flow to different areas and trying to establish a diurnal and demand pattern.

We located 16 areas -- or 16 points, not areas, 16 points where we wanted the flow meters installed. This discussion took place initially with representatives from environmental management division from headquarters, Marine Corps and Camp Lejeune staff in July 28th -- on a July 28th meeting at Camp Lejeune. And headquarters said to proceed with that.

As of -- in January, towards the end of January, all the flow meters were installed. It was ATSDR's technical staff, meaning myself and my staff, that a performance-based contract be used to install those; that is, install one and see any issues that may arise with it, how useful it may be. And then proceed to the next one or not proceed, as the case may be.

We were in a position that to let a contract of that size -- for ATSDR to let a contract would have required us to, at the minimum, advertise in the *Business Commerce Daily*, and you would have seen that taking six months or longer -- eight months. So at the time, it was decided that the Marine Corps would handle the procurement.

Apparently, they had a contract in place that would not require such a long time to get the flow meters installed for procurement. That was already in place,

whereas ATSDR would've had to advertise to the world basically on a size of that, 16 meters -- a contract containing 16 meters.

So that's why. The Marine Corps offered, and we accepted their offer for them to do the procurement and installation. So we were in the recommendation stage. We did recommend that it be performance based. All 16 were purchased, and all 16 were installed.

As of this past March, while they are operating, they are not calibrated. And we're still working on that. We have submitted a report, a detailed report, on every flow meter on what needs to be done to calibrate the flow meters so we can get reliable information. So the short answer to your question is: We have not obtained any reliable or useful information to date from the flow meters.

MR. ENSMINGER: What's the holdup with the calibration?

MR. MASLIA: Some technical issues. Number one, in the calibration process, certain valves have to be shut off to zero the meters out. And on the other side is ATSDR not having -- or I not having staff to actually -- as I alluded to, we don't have a field office there. So when questions need to be answered, we are not on site to specifically direct the work to do that.

We are not on site there full-time, and so it's a combination of installing field equipment and so us making trips back and forth. We have been told on a number of occasions that the flow meters have been calibrated. We have made trips up there, and when we try to QAQC them, they're not calibrated.

DR. JOHNSON: Let me digress and ask if anyone else from the public plans to make a statement.

(No audible response)

DR. JOHNSON: Seeing no hands raised, please, continue.

MR. ENSMINGER: Historical documentation: pumping records as to quantity, quality, distribution-system problems, well-field problems, infrastructure data on well construction, depth output, locations are by necessity to be furnished by the environmental management division of Marine Corps base Camp Lejeune or by their utility section.

Has ATSDR received all the materials it has specified that it would require? And if not, what is the explanation? And has ATSDR brought this matter of lack of cooperation to a -- to the attention of anybody else, such as headquarters of the Marine Corps?

MR. FAYE: Well, first of all, let's not make the presumption that there's been a lack of cooperation

because I wouldn't go that far. In a number of areas that are very critical, the Marine Corps has been extremely forthcoming and provided very useful information.

As far as the well data are concerned, between the information that we have obtained from the Marine Corps and from the U.S. Geological Survey, who, as I mentioned earlier, did two very comprehensive studies there in the late 1980s, we've got a -- we have -- ATSDR has a very -- what I would say a very substantially complete record of all of the wells that have been drilled at either Holcomb Boulevard, Hadnot Point, or Tarawa Terrace, or Camp Johnson, starting back in the early 1940s up to about 1987 or '88.

We do have additional -- well, several additional well records that have been completed at Camp Lejeune; very extensive records with contract numbers and whatever. Now, we have asked Camp Lejeune if -- we've asked them for some location data and other information about these wells that they've not provided yet. But in that regard, you know, that's only a half a dozen records.

Another thing I'd like to point out is the records provided to us relative to RI/FS studies and underground-storage tank removal studies at Tarawa Terrace have been very, very useful. And as far as I can tell, the records

provided by Camp Lejeune, which are in the dozens -- dozens of reports, are complete.

We would really like to have a similar contribution of those RI/FS and underground-storage tank removal reports, et cetera, from the -- for the Holcomb Boulevard area and the Hadnot Point area, and we've asked for that. But that's a large volume of information, and we haven't received it yet. But we hope we will in the future. In fact, very soon, I hope.

But as far as the well data are concerned, specifically, I think we have a very substantially complete record of what's available, of the data available.

MR. ENSMINGER: Listening earlier --

MR. FAYE: No. That doesn't -- that includes the well data in terms of, like, construction. That does not include operational information.

MR. ENSMINGER: Yeah. That's what I was just going to ask because earlier you stated that you didn't have near the information for, say, Hadnot Point that you did for Tarawa Terrace. I mean, that's the same organization. The same outfit that's running Tarawa Terrace is running Hadnot Point. So if they had good records for Tarawa Terrace, they should have good records for Hadnot Point water system as well.

MR. ASHTON: I'd like to --

COURT REPORTER: I need you to get to a microphone. Please identify yourself.

MR. ASHTON: I'm Brynn Ashton, and I've been really spearheading the effort from our environmental management division to provide the information. And in all cases, I think we've given -- we tried to provide you whatever we have. Recordkeeping is not consistent across Camp Lejeune. And there's been times where we might have some information in certain plants. We might not have as good information or organized as well in other plants.

So what we've tried to do is provide whatever we have, and, you know, the Commandant has made it very clear to us that we shall provide you with whatever information we have in as timely a manner as possible. If, at any time, it appears that we are not providing that information, it's just because it's not available or it's not organized. Or in some cases, we've scoured our records. We've found records that we did not realize were in existence. So in summary, we have the charge, we have the mission, to provide as much information as you ask in as timely a manner as possible.

MR. ENSMINGER: I have another question for you while you're up here. If that's the case, the plant account records --

MR. ASHTON: Yes.

MR. ENSMINGER: -- I know that EMD has a listing of all and has pulled all the well data and all of the water-system data off the plant account records, all the historical data. I know it exists because I used to call Rick Raines and get certain information from him when he was here. Why hasn't that been provided to them?

MR. ASHTON: Now, I think -- I think they will verify that we've provided them what we have. The plant account data is very minimal. It -- what it has is it has square footage of the buildings. It has years of construction. It has, you know, numbers of the facilities. It has certain category codes, and that -- you now, that is available through our plant account organization.

MR. ENSMINGER: I know.

MR. ASHTON: Some of it was not computerized. Some of it's in hard copy.

MR. ENSMINGER: I know.

MR. ASHTON: I think we've provided you what you've asked for on the plant account. And we've -- we actually have a point of contact that runs that section, and what we've done is we've provided the point of contact so you can get whatever information they have.

Again, you know, I'm not always proud of their -- the level of recordkeeping that we've done in the past. You

know, we've already alluded to some gaps in the knowledge. Whatever we have, whatever we can locate, we provide. And, you know, that's our charter. That's our charge from the highest level, from the Commandant, is that we be fully cooperative, that we provide whatever information we have. And we're routinely -- we're going through records as we speak. We've got volumes of records.

Morris will verify to the facts that we have this vault with, probably, 70,000 different drawings in it. And the vault dates back from the forties because, for example, Tarawa Terrace was built by a private contractor --

MR. ENSMINGER: Mm-hmm.

MR. ASHTON: -- the records are very spotty because we -- they weren't government records when the development was initially constructed. The air station, for example -- this isn't part of this study. But, you know, we had virtually no construction drawings from the early fifties from the air station. It was just discarded by somebody. That's the unfortunate environment that we're working with. But the one thing that, I guess, I'm here to say is that whatever support we can provide, whatever information we can provide, we try to provide that as soon as -- in as timely a manner as possible.

MR. ENSMINGER: Thank you.

DR. JOHNSON: Well, thank you for your comments. Do you have one more question, Mr. Ensminger?

MR. ENSMINGER: No. I have some -- I have some statements. The reason I am a bit skeptical of the Marine Corps or their personnel, as far as their involvement in this thing -- and you have to admit, Camp Lejeune, that -- or the people that represent Camp Lejeune now, today, what was done in the past at Lejeune regarding this situation, there's -- there have been some real atrocities committed down there by some people that provided ATSDR with incorrect water-system data, purposely. And when they were told to correct it, they did not do it.

And there was a repeated request by headquarters Marine Corps for you to correct it -- or not you, but your predecessors: Mr. Neil Paul to be exact. And he did nothing. And ATSDR went from 1993 to 2003 under the assumption that the Holcomb Boulevard water system provided water for all those housing areas on the main part of the base for the entire study period, which was '68 through '85 when, in fact, Hadnot Point provided that water up until 1973, August of '73. And that's by statement from Carl Baker from the plant account records.

So can you understand my skepticism? And you've got to understand that I lost a child. And I wish -- there's no way that I can relay to you what I feel and what my

daughter went through. And damn it, I want to know, and there's a lot of other people out there that want to know what happened to their kids. I want to know why my daughter went through the hell she went through. And if there's anybody that's withholding information or not providing correct information, I swear to God, if I find out about it, I'll do everything that is possible to make sure that they are dealt with.

DR. JOHNSON: We appreciate your comments, and we offer, certainly, our condolences in the loss of a child. We cannot fully appreciate your feelings, but we certainly commiserate with you and offer you our sympathies.

I have asked your comments and those from Mr. Townsend might be made part of this public meeting's record. I have suggested, Dr. Cibulas, that the agency provide a response to what are serious and important questions. And I hope that you feel that you've had a fair hearing and response to your questions today.

MR. ENSMINGER: Well, we'll see by the end of tomorrow.

DR. JOHNSON: Okay.

MR. ENSMINGER: Thank you.

DR. JOHNSON: Thank you again. I'd like to return to these eight questions and ask first of all, Mr. Maslia, we've got four through eight. Is there any priority here

in these -- priority of importance in these questions that remain?

MR. MASLIA: Okay. Let me reorient myself here; not really. They're of equal importance.

DR. JOHNSON: Okay. Let's turn to Question 4: Should ATSDR consider using a parameter estimation approach to assess parameter sensitivity? And I suggest that you -- that we ignore the second part of that question: when such a process should begin. Anyone want to take a bite on parameter estimation? Eric.

DR. LABOLLE: Are we referring to the distribution system model or the groundwater model at this point?

DR. JOHNSON: Groundwater.

MR. MASLIA: Groundwater.

DR. LABOLLE: Well, my primary concern would be with dealing with the uncertainty and variability in the subsurface with regards to parameter estimation. At this point in time, there is some preliminary characterization done and a model constructed. And the construction of the model -- and I think I voiced some of this in my premeeting comments -- kind of constrains one's characterization of the subsurface, which is considerably more variable. And the uncertainty in that is great. We have samples at locations, wells, borings, and such, but

no information between other than what we know of the geology.

And so the parameter estimation that you do is going to allow you to vary these parameters within the cells based upon the constraints of the model. And my concern -- not -- that's not a bad idea, but my concern would be that the model response is still constrained by the characterization that's in place and that there potentially be, in addition to, depending on the role of the groundwater model, of course, and the level of detail that it requires in order to improve the answer.

But my concern would be that not only there would be some parameter estimation, but also a way of addressing the uncertainty and variability in the subsurface beyond the constraints imposed by the current characterization, if necessary.

And that's going to be driven by the epi model, whether or not one needs to essentially get at multiple exposure scenarios in order to tease out the dose response. So if the epi model is very weak in a sense in terms of its correlation, the actual dose response, then one might need multiple exposure scenarios in order to find that. There's my primary concern. But, certainly, parameter estimation, I think, is a necessary step if,

indeed, one needs to refine the arrival curves to these wells.

MR. FAYE: I have no argument or really even any comment to say except that I agree with you, and we've always planned to use parameter estimation to the greatest extent that we possibly could. We've only done it recently -- or not recently. But with respect to the prepumping model, I spent quite a bit of time using PEST and UCODE to estimate -- to estimate that recharge rate. And frankly, I didn't get any better answers than just using the estimate that's published in several -- several papers. So -- but it's something that we definitely plan to deal with in the future.

DR. LABOLLE: There is one additional concern actually with regards to parameter estimation that I've been meaning to touch on at some point here which is: What data do you calibrate to? And I've noted from some of the slides you had up there that parameter estimation or the focus on the calibration has been on the hydraulic model, and that's used in the transport model. Now, to the extent that the parameter estimation could be used in combination for both the hydraulic and the transport model, I think that's quite important.

And the more recent data that's available on concentrations, unfortunately, probably doesn't overlap

with the time frame of interest and the time frame in which the model's been developed.

But if there was any plan to extend the model period forward over the later periods over which you have better information, there may be something to be gained from calibrating the transport model to probably the better data on concentrations in later time periods.

MR. FAYE: Oh, yeah. We would definitely be remiss if we ended our calibration in 1985. We would extend the calibration for the fate and transport to 1991, which is the last period that we actually have contaminant information at several supply wells. That's always been on the books to do that. I had another comment. It slipped my mind.

DR. LABOLLE: Is there --

MR. FAYE: The -- pardon?

DR. LABOLLE: Is there additional data after '91 also?

MR. FAYE: No; no; no. As Mr. Ensminger said and as I reiterated later in some of my comments, apparently, right after the wells were sampled during Operable Unit 1, the Operable Unit 1 study at ABC Cleaners, the Marine Corps destroyed the wells, literally. It grouted them up, took the hardware out, pumps, and grouted them up.

DR. LABOLLE: And there's no monitoring at the monitoring wells after that time period? Or is there?

MR. FAYE: No; no. I think actually the monitoring wells are gone as well.

DR. LABOLLE: Okay.

DR FAYE: Except for the immediate vicinity of ABC Cleaners because they have to -- they have to have some means of determining the efficiency of their remediation activity there at ABC Cleaners. So that's pretty much it. The -- as you saw, we would -- in order to -- in order to do some parameter estimation during this transient period, we would probably do some additional refinement on those so-called static water levels.

You saw the shotgun scatter diagram there, so that makes -- that makes the notion of parameter estimation a little -- a little difficult when you're trying to match that number of water levels plus that type of variability in the water levels. But it's definitely something that we -- that we'll deal with. And that was a good comment. Thank you.

DR. JOHNSON: Any further comments on that question?

DR. CLARK: One comment.

DR. JOHNSON: Yes; please, Bob.

DR. CLARK: It seems to me that, in addition to having data for parameter estimation, it would be nice to

be able to create an independent data set for validation of predictions. And I think that would be an essential part of the protocol for doing the regression estimations.

MR. FAYE: The only -- the only way we could do that would basically to be randomly select data from the -- from the -- from the total population of the database that we've got. We could do that.

DR. SINGH: I would like -- since it says no linear regression approach, you know, I think you should consider using more efficient and powerful parameter estimation techniques, such as GLUE, and especially in conjunction with the generic programming, your load times. I think that would be a better approach than only the regression approach, especially when you have such limited data.

MR. FAYE: Thank you.

DR. DOUGHERTY: One other comment is that, at least the way the language is usually used, parameter estimation assumes a model. And it seems to me that the model estimation, at least the submodel for source terms -- and getting ahead of our current topic -- tanks is perhaps more significant than some of the parameters that one might first think of going off and estimating. And my initial reaction is that the model estimation process, particularly at the source term, is more significant.

DR. JOHNSON: Okay. Let's move on to the next question. Should ATSDR consider using probabilistic analyses to assess the variability and uncertainty of model parameters and variability and uncertainty of contaminant concentrations at public supply wells? Are there public domain codes available that the panel would recommend using? Anyone want to bite on that one? Please.

DR. CLARK: I'll take a shot at it. Yeah. I think the idea of using probabilistic analysis and so forth is a good idea, but I'm wondering: You're having enough trouble dealing with just the -- with the deterministic model you're working with is -- wouldn't that add a level of complexity that goes way beyond where you could possibly go at this point?

MR. FAYE: That was your question, Morris. You answer it.

MR. MASLIA: Yes. Yes. That was a question posed in the early stages of the formulation of this panel, and we were trying to consider any and all topics that might be brought to the table. And obviously, the panel has sort of narrowed our focus into certain areas. And it may be just more than we can bite off at the present time. And I think, as David already appropriately pointed out, we may be talking more into model estimation as opposed to

parameter estimation, given the limited data that is available, and really find out how our model -- the effect on the performance of our model.

If I could just go back for a second when -- Eric, you were speaking about calibration for -- from the water-quality standpoint or from the transport standpoint in addition to the hydraulic. And I think we've taken -- and this gets into the distribution side.

But we've taken that approach, and that's one of the ideas that has driven us on the water-distribution side -- once we saw some of the hydraulic parameters of the distribution side -- to do tracer tests, realizing that if we were going to ever calibrate a distribution model that we would have to calibrate it to water-quality parameters, rather than just on the hydraulic side.

We would probably end up, at best, with a nonunique hydraulic solution; at the very best if we did not. So we are aware of that. Your point is well taken. We're probably at that step on the distribution side, and that's a step we need to look at from the groundwater side.

DR. LABOLLE: I think it might be important here to define what we mean by calibration to some extent because the previous question was with regards to parameter estimation for calibration. But in my mind, when I speak of calibration, I think we're talking the big picture,

including the source term, as David brought up, then including the recharge and everything else --

MR. MASLIA: Oh, absolutely.

DR. LABOLLE: -- that comes into play here, so...

And with regards to the use of specific models, I'm reluctant to advise ATSDR to necessarily embark on, for example, a geostatistical approach to -- although that's kind of what I was implying by my previous answer. I'm reluctant to specifically recommend that at this point in time until I understand more the role of the tanks, the mixing, and the distribution-system model, the time frame at which we know contamination was present at some of the wells relative to, you know, some of the uncertainty, and how much uncertainty can be tolerated in the epi model. I think that's going to become apparent over the next day and a half.

DR. WALSKI: Instead of using the word "probabilistic" analysis, I would just think in -- more in terms of sensitivity analysis. Find out what is the model sensitive to and focus on that parameter and not try to figure out every cell's hydraulic conductivity or anything like that. And you know, focus on the one or two things that really make a difference. And it's probably going to be source.

DR. DOUGHERTY: And the answer is -- focusing on the last part of the question, rather than the first part, the answer is yes. You should you use some probabilistic analysis for the impact at the -- it's not clear yet whether it's the individual wells or the blended well concentrations but on that metric. Yes.

DR. LABOLLE: Yeah. The answer -- if I can elaborate on what I said -- I was reluctant to provide recommendations for using geostatistics but certainly some sort of probabilistic analysis is going to have to be employed to consider the uncertainty in these arrival curves to the wells regardless of how well you know the source because although the source terms -- and the uncertainty in that is going to, you know, directly affect the arrival to these wells and the concentrations at which the PCE arrives.

The hydrogeologic uncertainty is an additional component that will make that highly uncertain as well and possibly on the order of a magnitude, an order of magnitude or more, maybe even two orders of magnitude, uncertainty in concentrations that arrive to these wells, even from the hydrogeologic uncertainty. And so constraining that, to the extent that you can, from the models, I think, is important.

DR. JOHNSON: Okay; moving on. How should ATSDR address the issue of lack of observed water-level data prior to 1974, reminding us that the epi study is from 1968 -- or covers 1968 through 1985?

MR. MASLIA: That should have been from '78. If you've been following the discussion all day, we don't have the data prior to '78.

MR. FAYE: Very few.

DR. JOHNSON: So that becomes a moot question.

MR. FAYE: No. But I think we've already addressed it in terms of the uncertainty discussions and the parameter estimation discussions. I think we just sort -- it would be a lot of repetition in response to that question, but that's no reason not to respond.

DR. JOHNSON: If you're happy, I'm happy. Any comments on --

MR. FAYE: Okay. I'm happy.

MR. MASLIA: The only comment I will -- I will make and I've had this initial discussion with Frank Bove, and he's actually prepared some, I guess, iterations or some initial analyses. And the discussion went along the line is: How much uncertainty or variability could the epi study tolerate in terms of if our arrival times are plus or minus a couple of months versus plus or minus six months versus plus or minus a couple of years?

And that's an issue. As I said, he's just prepared some preliminary analysis on, but that's something we need to sit down and discuss with them. That's the exact issue. So the fact that we don't have very many data prior to '78 brings that again to the forefront since they're starting the study in '68.

DR. KONIKOW: Do you have pumpage data from prior to 1978?

MR. FAYE: Yeah, we do, Lenny. We have periodic information for, perhaps -- well, not perhaps, for a particular year. Maybe, I think we have data for '71. We have data for '62. And, of course, the USGS, their data go to '75. I think we also have some '68 data, but these are just, you know, snapshots.

And -- but the point is -- and I think I made it earlier -- that because of the -- because of the utility of Tarawa Terrace, the housing was occupied 90 percent to 100 percent all the time. And that's borne out in the USGS data as well. I mean, we're looking at point -- averages of .95 MGD plus or minus 10 percent for, you know, well over a decade. And I think that was probably the case, you know, from the get go.

DR. KONIKOW: So really what you're saying is that if you can calibrate the model adequately for the times when you have water-level data --

MR. FAYE: Right. That's --
DR. KONIKOW: -- you could then run the model --
MR. FAYE: That's the whole plan.
DR. KONIKOW: -- impose the stresses --
MR. FAYE: Yep.
DR. KONIKOW: -- for the earlier time.
MR. FAYE: Right. That's the plan.
DR. KONIKOW: -- and that still leaves you with the issue of concentrations though.
MR. FAYE: Exactly; exactly. And the thing that we hope to be able to do is to have some good estimate of mass loading through time. It should be fairly constant except for the periods there that, like Mr. Ensminger was discussing during Vietnam, when there was -- when it was probably somewhat to greatly accelerated, the activities at ABC Cleaners.
But for all intents and purposes, it is a single source, and hopefully, maybe, perhaps from these tax records or other information that we'll be able to discover in the reasonably near future. We should be able to -- or we'll hopefully be able to get or to obtain some notion of the use at the source. That still doesn't really address what the loss -- what the percentage of loss was from their actual total use. So we'll just have to start out, make some estimates, do alternative

simulations, and hopefully arrive at a defensible, reasonable answer.

DR. KONIKOW: Well, I think what you're going to come up with is that there was some contamination there from the beginning of this --

MR. FAYE: Right.

DR. KONIKOW: -- epidemiological study.

MR. FAYE: Oh, yes.

DR. KONIKOW: And --

MR. FAYE: No question.

DR. KONIKOW: -- you may not be able to refine it down any more than we just said.

MR. FAYE: Maybe we can't; yeah. I don't know whether that precludes the attempt or not. That's hopefully what -- where we'll get -- gain some insights from you-all.

DR. JOHNSON: Okay. How should ATSDR address the issue of lack of monthly groundwater production data when monthly data are required for the epi study?

MR. FAYE: Well, let me say a few words about that too. We now have good monthly data back to 1980. All right? And we have -- prior to 19 -- 1980, we probably have, maybe, three, four, five snapshots in time of the well capacities because the well capacities have changed through time.

So what we can ultimately do -- what we possibly should be able to do, using the monthly data that we do have now from 1980 through 1984 and the well capacity data that we have for that time, possibly rate the -- that use as a factor of -- as a factor of capacity. And then, as the capacity changes back through the historical record, adjust that on a monthly basis. And knowing what the annual record is -- we know what monthly variability is now from the -- from the detailed records that we have for those four, five, six years -- develop a model of activity. Okay?

MR. MASLIA: One of the pieces of information that we've just recently obtained, which has been referred to, is this plant accountability record. I actually have a copy with it, and it goes from 1990 backwards 'til they started keeping the records.

What's in it is it lists -- for example, it lists the pump house or well house and treatment facility and anything by all the different water-plant areas at Camp Lejeune. It references a card number, which is my understanding how records are referenced to or kept in the vault at Camp Lejeune. That should -- at least, we'll make the attempt at going back there and pulling whatever information is in there.

Up until we got that information telling us or suggesting that we go into this storehouse of information and start looking someplace, it was like looking for a needle in a haystack. You don't know where to turn to look. At least now we have some directed means. Whether that yields useful information or not, I can't answer, but that may -- in fact, just this last week -- I think it was last Thursday or Friday -- I received from the EMD folks at Camp Lejeune the -- was it from the '80 to '80 --

MR. ASHTON: '84.

MR. MASLIA: '80 through '84 monthly production records by every water system. So this information is still coming in. And as we have -- as we refine -- excuse me -- our approach based on recommendations from this panel -- also I think that goes hand-in-hand with hopefully obtaining additional data we may find. In other words, we have not given up on trying to locate the earlier information.

DR. JOHNSON: Okay. Anything else? Lastly, Question 8: Is it sufficient to use an annual average recharge or infiltration rate and assess climatic conditions to derive monthly recharge rates? Are other methods or techniques available to derive monthly recharge data? Does anyone know?

DR. CLARK: [off microphone]

COURT REPORTER: Sir, I need you to use the microphone.

DR. CLARK: I'm sorry. Could one use some of the meteorological data we discussed to get estimates?

MR. FAYE: Yes. That's our plan now. We have monthly rainfall, pan evaporation records for the entire period of interest, starting in the early fifties and going up into the nineties. And once we can decide on this baseline annual recharge, whatever it is -- 14 inches, 13 inches, 15, something like that.

Whatever that is, then we can use that -- and we compare that then to the -- we have -- what we'll have from that -- from that long period of meteorological record, we'll have an -- a long-term average annual rainfall as well. So we can equate that 14 inches of recharge to the long-term average rainfall. And then, using the monthly data, we can prorate that out.

We can say, well, for 1963 the recharge -- the annual recharge was only 10 inches and prorate that out on a monthly basis, using the meteorological record. 1975, it was 16 inches and prorate that out, using the meteorological record. And hopefully, we can develop a recharge schedule for the various stress periods that way. It's not -- it's not, you know, it's not rocket science,

but it is somewhat practical and common-sensely and straightforward. So hopefully, it might work.

DR. CLARK: Can you get an estimate for changes in soil permeability over that period of time?

MR. FAYE: There may be some agricultural records at an experiment station somewhere down there in the coastal plain where they -- where they collect those -- that information, I guess, almost daily, particularly during dry periods. We haven't looked for it.

DR. DOUGHERTY: The only comment I have with respect to using the preset and then generating the variations of the record is that that may be excessively rough compared to the infiltration function at -- as it accretes to the groundwater system. So it may be useful to -- basically the unsaturated zone acts as a buffer and --

MR. FAYE: Sure.

DR. DOUGHERTY: -- and a smoother, so it may be useful to use a very simplistic, one-dimensional model, representative of characteristic depths to groundwater --

MR. FAYE: Oh.

DR. DOUGHERTY: -- to reduce the roughness.

MR. FAYE: Mm-hmm. And then what would you -- you would -- you would bleed off the rainfall with some estimate of ET or loss, using, what, pan evaporation data or something like that?

DR. DOUGHERTY: That's one approach. The other approach may be to do a simple, straightforward extension of what you're doing now.

MR. FAYE: Oh, okay.

DR. DOUGHERTY: You have an average --

MR. FAYE: Okay.

DR. DOUGHERTY: -- from the average prorate. That's the loading to the top of your reactor.

MR. FAYE: Right; right. And the advantage of what you're saying just because we think we got 14 inches of recharge or maybe the 1 inch of recharge during a particular month -- because of the thickness of the unsaturated zone, the water table may not see that for another month or another two months.

DR. DOUGHERTY: Right. The unsaturated zone acts as --

MR. FAYE: Yeah.

DR. DOUGHERTY: -- as bank storage.

MR. FAYE: Yeah. And the advantage of what you're saying would allow us to look at that antecedent condition pretty nicely.

DR. DOUGHERTY: Perhaps. The other advantage is that it may smooth out some rewetting problems that you may have because it's smoother rather than rougher.

MR. FAYE: Oh, yeah; right; okay.

DR. LABOLLE: You might try -- with regards to that, you might try the -- I think it's been released. But one of the researchers in our office was developing --

COURT REPORTER: Can you get nearer your microphone, please.

DR. LABOLLE: -- the sat/unsat package for Modflow. And it's not a full unsaturated code, so it doesn't have its complexities that you'd -- that you would normally associate with that --

MR. FAYE: Well, that's good.

DR. LABOLLE: -- an enigmatic wave --

MR. FAYE: Okay.

DR. LABOLLE: -- approach.

MR. FAYE: Yeah.

DR. LABOLLE: And it will provide the buffering that you're looking for. It's essentially, you know, a modified recharge.

MR. FAYE: Oh, that would be nice. What's this person's name?

DR. LABOLLE: That's the -- Dave Prudic is working on that with Richard --

MR. FAYE: Oh, yeah, I know Dave.

MR. MASLIA: Oh, we know Dave.

MR. FAYE: He's a personal friend of mine.

COURT REPORTER: One at a time, please.

DR. LABOLLE: Rich and Dave are the two --

MR. FAYE: Okay.

DR. LABOLLE: -- that have been developing that, so --

MR. FAYE: Oh, okay.

DR. LABOLLE: I think it's either been released or it's in testing, one or the other.

MR. FAYE: All right. Well, it's time to harass Dave.

MR. MASLIA: Lenny, would you know anything -- would you know anything about if that's been officially released by the survey?

DR. KONIKOW: To the best of my knowledge, it's not officially released yet.

MR. MASLIA: Okay.

DR. JOHNSON: Okay. We have plodded through these eight questions, and I offer the panel the opportunity to further elaborate on any point, something you, maybe, have forgotten and wished you had brought up as an earlier discussion. But this is going to be pretty much the conclusion of comments on the groundwater modeling. Anything that any panelist wishes? Please, James.

DR. UBER: Well, I just -- I'm no groundwater modeler at all, but I've heard a few people talk about source terms. And I just offer this as an idea for it to be shot

down, I guess. I wonder whether some more time should be spent on working your way back to the source, to your, you know, your hardest number, which I guess is your estimate of how much PERC they used on a monthly basis.

So in other words, I mean, I don't know how a dry-cleaner operates and how much they lose --

MR. FAYE: Well, we don't either.

DR. UBER: -- and how much is diluted with other -- with water as it goes into the septic system and whatnot. But should more effort be spent on modeling that process?

MR. MASLIA: I think -- if I can do that one.

MR. FAYE: Have at it.

MR. MASLIA: That's really -- and this may be an inappropriate term, but I'm going to use it anyway. I can get shot down. That's really a facilities management-type question that you're asking. How was the facility managed, and can we glean any information as far as how we classify or quantify the source that goes into our groundwater model?

In other words -- and that, I think, goes back to this data-discovery issue. Can we pull tax records? Can we perhaps find -- and I don't know the issue. But if you look at deliveries, deliveries to the dry-cleaner on how much they use, we should see an upswing during the Vietnam period, obviously.

And then perhaps through the -- there's a dry-cleaner -- National Dry-Cleaners Association. Because my dry-cleaners -- I asked him once about PERC, and he gave some handout from them. So I know they have a national organization. They may, in fact, have some information we have not looked on on typical uses, historic uses. That's an area, I agree, I think we need to really look at.

DR. WALSKI: So related to this, we're doing all this sophisticated stuff, going back through tax records and all that, why don't we just talk to the guy that ran ABC Cleaners? I mean, get somebody who was the manager and interview that person and find out what they did, I mean --

UNIDIENTIFIED SPEAKER: Because he's dead.

COURT REPORTER: Either at the mike, or (laughter)...

DR. WALSKI: If he's dead, then I think one of his employees or somebody should know what went on there. There should be somebody who worked there that's still alive.

MR. FAYE: I think we're also dealing with, Tom, something you pointed out a few minutes ago with regard to the operation of these water-treatment plants. And that is, you know, there's a broad spectrum of the way folks do things, and I think -- and we had two -- we have two examples right there.

We have the ABC Cleaners, who were, obviously, exceptionally sloppy, to put it kindly, and we have this Globarama place, who was very -- they were very efficient in their operations and how they -- how they tracked their and collected their PCE waste. So, yeah, we need to try to find out as much as we can about that. And all of that affects the source term, and there's just no denying -- and we wouldn't that -- the source term is a critical, critical, critical feature of the fate and transport model.

DR. LABOLLE: You might want to look at one of these other simple models for looking at a dissolving source like that, you know, a DNAPL, like we're dealing with here.

MR. FAYE: Mm-hmm.

DR. LABOLLE: And I've actually run some of these in the past. I forgotten the names if it. Something called 3-D? Does that sound --

MR. FAYE: There's something called Fate 5. There's a number of them out there.

DR. LABOLLE: And, you know, that may be helpful, I think, in --

MR. FAYE: Mm-hmm.

DR. LABOLLE: -- because, you know, what's been mentioned is one aspect, which is facilities operation.

But then below that, you know, you've got the unsaturated zone. You've got the source entering in there. And you're looking at the saturated zone, not the unsaturated zone.

MR. FAYE: Right.

DR. LABOLLE: So it might be useful in helping to refine what the source may have looked like once you get a handle on how much is entering the subsurface.

DR. JOHNSON: Mr. Ensminger.

MR. ENSMINGER: I just wanted to add one thing. I know that depositions were taken prior to Mr. Meltz's death by the EPA and some different law firms. And those are available.

MR. FAYE: Do you know where?

MR. ENSMINGER: Yes. I'll tell you.

DR. JOHNSON: Okay. Thank you for your comment. Anything else on groundwater?

(No audible response)

DR. JOHNSON: Looking at tomorrow, let me bring to your attention that we begin at eight a.m., not 8:30. So there's a time change, so be here a few minutes before eight. We will begin, Morris, with your presentation on the water-distribution system, an update on that work, and then go from there into the set of questions that the agency has brought forward.

As a matter of, perhaps, a take-home assignment to the panelists, we're going to be talking about these four charges. And clearly, we've already discussed some of this. And tomorrow at the working lunch, we need to begin formulating some specific responses to these four charges. And I would ask that you simply look at these four charges tonight, maybe put a few notes in the margin. And that will help us perhaps go through these in a more efficient fashion tomorrow.

With regard to the hotel, is there transportation provided this evening as well as tomorrow? It's a very accommodating hotel.

MR. MASLIA: There probably is. If there's anyone out in the lobby -- you mean going back to or going out to a restaurant?

MR. MASLIA: Going --

DR. JOHNSON: All of the above; yes.

MR. MASLIA: The hotel is very accommodating, and I will see if anyone's out in the hallway to answer that question.

But if I -- if I might just -- about a 60-second point here is, again, on behalf of the technical staff -- and I assume I won't get beat over the head by agency management for speaking for the agency, although Bill's backing his chair up right now, so maybe I shouldn't. We

do very much appreciate your input. It's very useable. It's from people who've seen a variety of cases, both public and private contamination cases.

One of the things we take into consideration -- for example, if we modify or go down a different path, taking the information that you have provided us, we still need to provide our other audience, the public and others, a technical reason why we have chosen to change direction. In other words, so that may still require us to say, "Well, we did a cursory review of Hadnot Point, and, based on recommendations from the panel and what we're seeing right now, we're not going there any longer."

And that's just, for those who are not familiar with the way ATSDR operates, we do have this other audience to, at least, you know, address or at least acknowledge their questions. So that's the other side to that. You're obviously not charged with, but our mission is charged with. So while some of these questions may seem like why did they ask these questions or why are they posing it, the answer may be obvious. We do -- we're posing them because we have another audience to acknowledge and to provide respectful answers for. So we do appreciate your contributions and look forward to continuing down with the distribution side tomorrow.

DR. JOHNSON: May we leave our materials in this room?

MR. MASLIA: Absolutely. It'll be locked up.

DR. JOHNSON: Okay. Anyone want to say anything?

(No audible response)

DR. JOHNSON: If not, thank you for a good day.

(Whereupon, the proceeding was adjourned at approximately 5:08 p.m.)